THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. XLIX.—*On the Geological History of the Gulf of Mexico;* by E. W. HILGARD, of the University of Mississippi.* With a Map.

THE colored outline map before you, without much pretension to accuracy of detail, shows the general geological features of the great embayment, once a portion of the Gulf of Mexico, whose axis is now marked by the course of the Mississippi river, from southern Illinois to its mouth. I have compiled this map from the best data now extant, accessible to me, with a view to the better elucidation of the succession and character of geological events; and especially with a hope of bringing to bear upon the later formations of the interior of the continent, the chronological record here left by the retiring waters of the sea. Marine deposits being better understood and more available for general comparison and conclusions than those of inland lakes, the series here shown would seem, by its original connection with the interior basins, to promise a key to the determination of equivalence in time, that, in view of the violent disturbances in the Rocky Mountain region, it might be difficult to find in that portion of the continent.

The subject matter of the present communication is, for the greater part, embraced in publications made by myself during the past ten years; and to these publications I must refer for the corroborative detail, which in this general summary would be out of place.

* Read before the American Association for the Advancement of Science, at Indianapolis, August, 1871.

Cretaceous period.

The most ancient shore-line of the Mississippi embayment is formed mainly by the various stages of the Carboniferous rocks. It is only in eastern Alabama and the adjoining portion of Georgia, that the Silurian and metamorphic rocks formed the shore of the Cretaceous seas. The deposits of the latter period have been traced by Safford up to the Kentucky line, along the western foot of the paleozoic ridge which compels the Tennessee river to make its long détour northward; and the Cretaceous outcrop doubtless extends, in a northwesterly direction, some distance into Kentucky.

In the portion of this belt embraced within the States of Mississippi and Alabama, the dip is sensibly at right angles to the trend (i. e., between W. and S.) at the rate of twenty to twenty-five feet per mile. In its southerly portion especially, the Cretaceous beds are very distinctly divided into three principal stages, viz:

1. A lower one 300 to 400 feet thick—*Coffee group* of Safford (Eutaw group *mihi*)—consisting of non-calcareous sands, and blue or reddish laminated clays, with occasional beds of lignite, and but very rarely (Finch's Ferry in Alabama) marine fossils, silicified; corresponds doubtless to Hayden's Dakota group, including, perhaps, in its upper part the equivalents of the Fort Benton group, to whose fossils those of Finch's Ferry, collected by Tuomey, bear a close analogy.

2. The middle or Rotten limestone group, not less then 1,200 feet in maximum thickness. Soft, mostly somewhat clayey, whitish, micro-crystalline limestones and calcareous clays; very uniform on the whole, if we except the locally important, but not generally extant, feature of the "Tombigby Sand," the special home of Inocerami, Selachians, and gigantic Ammonites.

3. Ripley group: crystalline, sandy limestones, alternating with dark colored glauconitic marls containing finely preserved fossils. Thickness 300 to 350 feet. Equivalent of the highest bed of the Cretaceous of New Jersey, and doubtless of Hayden's Fox Hills beds.

How far the Rotten limestone, as a whole, may be considered as embracing the whole of the series intervening between Hayden's Fox Hills beds and Dakota group, remains to be shown. The fauna of the Tombigby sand sub-group is distinguished, as already stated, by the great number, both of individuals and species of Inocerami, and of remains of (chiefly Selachian) fishes, wherein it corresponds to Hayden's Niobrara division; its fossils have been partly named and described (somewhat imperfectly) by Tuomey.* According to Hayden's view of the New Jersey

* Proceed. Philad. Acad.

equivalents, the Rotten limestone would be represented by his Fort Pierre group.

The distinctive features of these several groups become less marked the farther we advance northward, even in Mississippi. Non-fossiliferous or lignitic clays and sands mingle with the marine strata; and become altogether predominant, it would seem, near the northern termination of the outcrop.*

West of the Mississippi, the continuous Cretaceous outcrop does not extend as far northward as on the east side, by some 150 miles. Nor have the more ancient lignitiferous beds (Coffee group) been observed there, with certainty, within the limits of this map. The Cretaceous area of Arkansas, according to Owen's description, falls almost altogether within the limits of the Rotten limestone group; and the same seems to be true of the greater part of the Cretaceous area of northern and middle Texas. Nevertheless, the series of isolated Cretaceous outliers, which traverses Louisiana from the head of Lake Bisteneau in a S.S.E. direction, terminating probably in the great rock-salt mass of Petite Anse†, exhibit the main characteristics of the Ripley group; while deep borings have demonstrated the presence, for a thousand feet beneath, of the uniform Rotten limestone, such as it exists on the prairies of Mississippi and Alabama. I have elsewhere‡ stated the stratigraphical as well as lithological reasons which induce me to consider both the rock-salt of Petite Anse, and the sulphur and gypsum deposits of Calcasieu, as lying within the limits of the Cretaceous formations.

The data given by D. D. Owen seem to assign to the Cretaceous strata of Arkansas a dip S. or slightly W. of S. The outliers in Louisiana are too limited in extent for determinations of dip; but it can scarcely be doubted that they represent the summits of a (more or less interrupted) ancient ridge, a kind of "backbone" to the State of Louisiana, whose resistence to denudation has measurably influenced the nature and conformation of subsequent deposits. It is fair to presume that from this ridge the strata dip toward the axis of the Mississippi Valley, to meet those on the opposite side; and the depth at which these beds are found in the Calcasieu bores, seems to indicate, on the western slope, a south-southwesterly dip of three to four feet per mile. A glance at the map shows, nevertheless, that the general form of the northern Gulf shore was not materially influenced by the existence of this axis of elevation, which probably was marked merely by a series of disconnected islands in the early Tertiary sea that, after the emergence

* Fide Safford.

† Indicated on the map by the localities of Petite Anse, Chicot, Winfield and Bisteneau.

‡ This Journal, Nov., 1869, p. 345.

of the immence Cretaceous area, already prefigured the present Gulf of Mexico.

It would be bootless to speculate, at this early moment, on the precise origin of the great rock-salt, gypsum and sulphur deposits. That the prominent event of the epoch—the emergence of an extensive sea-bottom—afforded abundant opportunity for the accumulation of the two former substances, is obvious enough; it would seem to pre-suppose, however, a temporary or partial isolation at least from the general ocean, analogous to that which, apparently, occurred in later times. But as regards the sulphur, its ordinary co-occurrence with gypsum would hardly seem to afford a sufficient precedent for the present case; unless we assume the concurrent influence of volcanic or other "abysso-dynamic" agencies.

Tertiary period.

It will be perceived that during the Tertiary period, the northern Gulf shore receded from its extreme northern limits in southern Illinois and Missouri, to a shore-line which, though running near the latitude of Bâton Rouge, is not far from parallel to the present one, if we ignore the extreme projection of the Mississippi delta. This rapid filling-in of the embayment, no less than the character of the deposits, prove that the depth of water was not great; especially in the remoter portions, where lignitic and lignito-gypseous deposits very sparingly interspersed with small marine beds (the remnants of estuaries) from the predominant material. Similar alternations of materials occur, in fact, throughout the older Tertiary deposits of the southwest; and hence, the divisions marked off by difference of color on the map, as "lignitic" and "marine" Tertiary, respectively, must be taken very much *cum grano salis.* Except only in southern Arkansas, few marine beds of any notable extent occur outside (i. e., north of) the limit of the area indicated as marine. But in northern Louisiana, where the dip is very slight, lignitiferous strata are altogether predominant on the surface; although the marine seem to underlie everywhere at no great depth, and in numerous localities crop out on the surface; forming, according to Hopkins, distinct beaches around some of the Cretaceous outliers mentioned above.

So far, indeed, from considering the predominantly lignitiferous area as representing a period distinct from the older marine Tertiary, I have little doubt that the larger portion, if not all, of the beds I have heretofore designated as the Northern Lignitic (and Flatwoods clay) group (Lagrange and Porter's Creek groups of Safford) are the strict equivalents in time of the oldest marine beds observed in South Carolina and Alabama, and designated

by Tuomey as the Buhrstone group ("Siliceous Claiborne" of my Miss. Report). The lithological continuity of the bed-rocks of this group along the eastern border of the Tertiary, supported to some extent by paleontological evidence, strongly inclined me to this opinion ten years ago;* and it has received a strong confirmation from the latest observations of Dr. E. A. Smith, of the Miss. Geol. Survey, who has found the same rocks, substantially, continuous along the southern border of the lignitic area, nearly to the Mississippi bluff. At the same time, Safford mentions the occurrence of similar beds on the border of the Cretaceous, in Tennessee. The inference is inevitable that no beds outcropping in the fork of these two marine branches can be anterior in time; the interconnection, in fact, is such as to render the supposition that there can be any material difference of age almost stratigraphically impossible. The exclusively lignitic character of the central portion must, therefore, be ascribed rather to the inaccessibility of that region to the waters of the sea during their deposition; perhaps in consequence of a change of level, by which the upper portion of the embayment, from about the mouth of the Arkansas to Cairo, was converted, for the time being, into a littoral marsh.

In Arkansas, nevertheless, small marine beds are more liberally interspersed among the lignitic clays, than is the case east of the Mississippi; and some of those mentioned by Owen as occurring on the territory here laid down as chiefly lignitic, are obviously more closely related to the celebrated Claiborne shell-bed than to the Buhrstone group. The latter group does not, in fact, appear characteristically developed anywhere west of the river, so far as I know; and the occurrence of somewhat extensive marine Tertiary outliers on the Cretaceous territory of Arkansas, as well as of lignitic beds on that of Texas (e. g., the Cross Timbers, as approximately laid down on the map), proves that although the deeper water of the embayment followed substantially the lines of trend shown on the map, yet there still existed at that time a connection, in a northwesterly direction, of the Gulf waters with those of the great interior basin of the West.

That this connection should not be uninterruptedly traceable at the present time, is not surprising when we consider the shallowness of the connecting trough, as demonstrated by the inconsiderable thickness of the deposits, that of course greatly favored their removal by the subsequent events of the Quaternary period. Nevertheless, enough seems to remain of these deposits to form a chain by which, with the aid of paleobotany, the equivalents in time of the Buhrstone and Claiborne marine groups, at least, can be determined among the fresh or brackish-

* Miss. Rep., 1860, §§ 162 and ff. 188, etc.

water beds of the interior. And with these as fixed horizons to start from, aided also by the flora of the subordinate lignite beds of the later (Jackson and Vicksburg) stages, we may hope to establish a comparative chronological scale through which the parallelisms with more distant regions, and later times, may be established even for the much-discussed Tertiary beds of the Great West.

My present purpose scarcely requires that I should more than allude to the detail of the later stages of the older Tertiary, which I have not distinguished on the general map, in order not to obscure too much the general features. But a detailed map shows both the successive decrease in the width of their outcrops, and the regularly diminishing convexity of the Gulf-outline. I may also add that as we recede from the vertex of the embayment, and eastward from its axis, there is a regular increase of deep-sea features, the lignitic facies becoming more and more subordinate; yet, by its persistent recurrence seeming to intimate the occurrence, if not of oscillations, at least of local variations of depth; dependent, perhaps, upon corresponding changes in river mouths. In Alabama, the lignitic feature is almost suppressed, the marine stages overlying each other directly, as a rule; while west of the Mississippi, it becomes more and more pronounced as we advance westward, so that all but the latest portion of the Jackson, and most of the (much diminished) Vicksburg sea, here represented, appears to have been an intricate maze of everglades and shallow estuaries. That this state of things is intimately connected with the existence of the Cretaceous "backbone" of Louisiana on one hand, and the decided southward dip of the same formation in Alabama on the other, can scarcely be doubted. At the same time, let it be remembered that both east and west of the Mississippi, from the Chattahoochee to the Sabine, the older Tertiary period closes with a decided prevalence, in the Vicksburg limestones, of the deep-sea character; and thus far, the geological history of the gulf does not exhibit any phenomena whose parallel may not, *mutatis mutandis*, be found on the coast of the Carolinas. Moreover, the transition from the oldest to the more modern (Vicksburg) fauna is so gradual, gaps existing in Alabama and Mississippi being completely filled by transition strata observed by Hopkins and myself in Louisiana, that any attempt at subdivision into eocene and "oligocene" must draw altogether artificial lines of demarcation.

But while on the Atlantic coast we meet, in the Miocene and Pliocene strata of Maryland, Virginia and the Carolinas, a gradual approximation to, and admixture of, modern marine forms, the Vicksburg epoch closes abruptly, so far as the Gulf of Mexico is concerned, the marine Tertiary series. The geolo-

gically as well as agriculturally barren rocks of the *Grand Gulf* age lack all analogy outside of the Gulf basin, unless it be those of the Bad Lands of Nebraska, and especially the White River beds, to which they bear an extraordinary lithological (as well as in some respects, stratigraphical) resemblance. However little this circumstance may prove as regards equivalence in time, we must, nevertheless, not forget that, as the only representative of the geological period intervening between the eocene and the Drift on the Gulf shores, the Grand Gulf group almost necessarily embraces among its equivalents these very beds; and since Marsh has found the latter so much farther south than they were supposed to exist but a short time ago, it is not altogether impossible that more direct relations between the two may yet be proven. The Grand Gulf rocks form the highest ridges of Louisiana as well as of south Mississippi, falling off rather abruptly into the level prairie country of the marine Tertiary. But even these ridges are capped by the ferruginous sandstone of the southern drift, which may have been instrumental not only in greatly diminishing their height, but also in sweeping away the links connecting them with the interior; as has unquestionably happened with regard to the older Tertiary.

Be that as it may, whether actual connection existed or not, we cannot escape the conclusion that analogous circumstances were required to produce analogous deposits. Foremost among these was *the exclusion of the sea;* nor can we account for the extreme scarcity of both animal and vegetable remains, that scarcely leaves us a hope of direct identification, unless upon the supposition that the water which deposited these beds was, take it altogether, too fresh for a salt-water fauna, yet too salt to admit of a fresh-water population. The solitary fragment of a turtle, recognized by Prof. Marsh, is all that has so far rewarded my many years' search for zoögene fossils in this formation; and with the exception of a single locality in Mississippi, not yet fully explored, the prospect for recognizable fossil plants is about equally discouraging.

I confess that such absolute dearth of life rather staggers my belief; and my later observations on the deposits of the Port Hudson (Quaternary) beds have led me to conclude, that in some degree this absence of life is only apparent; and that the *calcareous concretions*, so abundant in some of the clay strata of the formation, are but the substance of perhaps a very diversified fauna, whose calcareous portions have been thus transformed by maceration.* The calcareous ingredient, however, occurs only in the lower, clayey division of the series; and it is sufficiently remarkable that the fine sand- and clay-stones of the

* This Jour., Jan., 1869, p. 81; Ibid., Nov., 1869, p. 338.

upper division should have preserved no vestiges of either animal or vegetable life.

I do not see how, in view of the nature, thickness (about 250 feet) and wide distribution of this formation, the inference can be avoided that during the whole or a part of the interval between the Vicksburg and Drift ages, the Mexican Gulf was, by some means, isolated from the Atlantic ocean; or that at least its communication, perhaps across the still submerged peninsula of Florida, was so imperfect as to render the influx from the interior of the continent predominant over the original supply of sea-water. An upheaval of the northern borders of the Caribbean basin could easily accomplish this result, so long as the deep channels excavated by the Gulf stream in the strait of Yucatan, as well as those of Florida, did not yet exist. Too little is as yet known with accuracy regarding the geology of the nearer Antilles and Yucatan, to determine whether they bear the marks of the event recorded by the Grand Gulf rocks on the northern shore of the basin. The observations of Mr. Gabb in Sto. Domingo, and of the English geologists in Jamaica, seem to indicate the existence there of marine Miocene and Pliocene tertiaries, which are altogether unrepresented in the waters of the Gulf. It has been suggested to me that beds of that character may be covered by the Grand Gulf and later beds of the Gulf coast. I willingly leave the onus of proof on that score to those who may think such an assumption desirable. But were this the case, the necessity for assuming the cutting off of the Gulf basin from the Atlantic, on account of the existence of the Grand Gulf rocks, would be none the less cogent; for I doubt whether any geologist, upon full consideration of the facts, would for a moment entertain the idea that like the "Northern Lignitic" of the older Tertiary, the Grand Gulf beds could be explained away as a mere littoral formation.

It is worthy of remark that while east of the Mississippi, the peculiar sand- and clay-stones of this group are confined to the northwesterly portion of its area of occurrence, in Louisiana and eastern Texas these rocks are altogether predominant, especially along the northern (or landward) border, the clays being subordinate.

Quaternary beds.

The Grand Gulf rocks are almost everywhere immediately overlaid by the deposits of the stratified Drift or Orange Sand. Of course it overlies equally, as a rule, the more ancient formations (except where, from causes not always readily imagined, it seems to have been subsequently removed) up to the limits of the Paleozoic. Beyond these, its occurrence is more or less localized in conformity with the larger valleys, as observed by

Safford in Middle and Eastern Tennessee, and by Tuomey and myself in Alabama. The same is true, apparently of the larger channels of Texas. But within the limits of the Mississippi embayment, it constitutes one huge delta-shaped mass, covering the entire Tertiary, and a large portion of the Cretaceous area, to a depth varying from a few feet to over two hundred; on an average, perhaps, between sixty and one hundred feet. Its predominant material is orange or reddish, rounded sand, mostly ferruginous, of various degrees of induration, with subordinate beds of clay, and enormous gravel-streams, evidently denoting ancient channels.* Its beds disappear beneath those of the Port Hudson age about concurrently with those of the Grand Gulf era; and consequently, it cannot well be independently represented on the map.

I have heretofore† shown that in order to explain the phenomena offered by the Orange Sand of Mississippi and Louisiana, it seems necessary to assume that prior to its deposition, the Gulf coast suffered an elevation to the extent of at least 450 feet above its present level, accompanied by a much greater uprising near the head of the waters. This elevation was succeeded, during the "Champlain" epoch, by a slow depression to at least twice that amount; and finally, during the Terrace epoch, a re-elevation to at least the extent of 450 feet took place. These figures are *minima*, if we regard the sea- or rather the Gulf-level to have remained constant. But if, as seems necessary to assume, the Gulf was an isolated basin during the Grand Gulf era, it might possibly have been elevated as a whole, and the zero-point of the scale would be changed upward, accordingly. The same, in a reverse direction, would be true if it could be assumed that the occurrence of the glacial epoch sensibly affected the general level of the ocean.

Be that as it may, the gravel is composed of northern rocks, disposed in belts, of which one occupies the main axis of the embayment, while others mark outlets now closed; and the extensive denudation and violent plowing-up of the more ancient formations, clearly proves the occurrence of an immense flow of waters southward, which in the main channels moved pebbles of many pounds weight; while between these, the deposition of the finer materials took place in more quiet waters.

That these events were not of a local character; that on the contrary, the phenomena observed in the Southern States are but the necessary consequence and complement of the Drift phenomena at the North, hardly requires discussion; but it is time that these facts were more generally understood and taken into account by American geologists, and that the Ohio should

* Miss. Rep., 1860, this Jour., May, 1866, and other papers above referred to.
† This Jour., Nov., 1869, p. 335.

cease to be proclaimed the southern limit of the "Drift." A kind of settled prejudice on this point seems to have obscured the vision of more than one observer, in consequence whereof these deposits have been claimed, over and again, as portions of every formation existing in the southwest, from the Carboniferous to the Bluff or Loess. Thus, in northeastern Texas, they have been accounted of Tertiary age; the "Tertiary iron ores" of that region being precisely the same as those of the "Orange Sand" of Louisiana and Mississippi. The same is doubtless true, according to Safford, of the "Iron ore banks" of Tennessee, associated with the "Ore region gravel," precisely as is the case in northeast Mississippi. In Arkansas, Crowley's ridge, a peculiarly characteristic Orange Sand ridge abutting on the Mississippi river at Helena, has been properly accounted of Quaternary age by D. D. Owen; but he associates with it the underlying lignitic beds, which it is impossible to distinguish from those cropping out directly opposite, on the Mississippi river bluff, in Mississippi and Tennessee; and which are directly traceable to their connection with the oldest Tertiary.

Tuomey first asserted the existence of this southern Drift from Alabama to South Carolina, and conjectured its equivalence to the beds which underlie the cities of Richmond, Washington and Baltimore. I ascertained its wide prevalence in the States of Mississippi, Louisiana and Texas, and identified with it the superficial beds observed by Owen in Arkansas and southwestern Kentucky, and by Safford in Tennessee. So close and cogent a connection was thus established between it and the "modified Drift" of the Northwest, that I can no longer doubt its equivalence, whatever may be the precise mode of origin assigned to it. The "Eastern gravel" streams observed by Safford in the mountains of Tennessee, and no less by Kerr in North Carolina, have their counterparts in the rivers of Texas, and in the great pebble-belts of the Mississippi embayment.

But it will be difficult to combine into a harmonious whole the widely differing observations and opinions of geologists on the vexed Drift question, unless some agreement is come to as to the precise meaning of the word. Let it be understood that the term Drift, *unqualified*, shall embrace all the deposits formed between the end of the Tertiary period and the beginning of the Champlain era of depression, when "drifting" ceased on this continent, outside of river channels, and that within this *Drift period* are embraced, whether as consecutive, or more or less simultaneous, but genetically distinct formations, the Glacier-drift or moraines; the Iceberg (or "glacial") drift of the Northwest; and finally the "modified," or rather, *stratified* Drift* of

* The name "modified Drift" would be altogether inapplicable to a large part of the southern Drift, which is to a large extent "modified" Cretaceous and Tertiary material, only re-stratified, Drift-fashion.

the Western, South Atlantic and Gulf States. It will then become possible, by a comparison of the really cognate phenomena, to trace more definitely the history, both general and local, of that turbulent period, without the confusion attending the use of a word to which each observer attaches, more or less, a different meaning.

If it be fully ascertained that in its lower course the Ohio is (sensibly) the extreme southern limit of the "glacial" Drift, while the stratified Drift is substantially continuous from the lakes to the Gulf shore, we have before us a definite problem as to the causes, that can probably be solved by a close examination of the critical region, viz: the southern border of the Paleozoic in the Mississippi Valley. The general relations of the Drift to the Allegheny range are thus far, unfortunately, involved in great obscurity; yet a knowledge of these, especially on the western slope, seems almost a necessary condition precedent of any probable hypothesis regarding the history of the Drift period West and South. It is only thus that the possible existence of an ancient barrier across the Mississippi Valley, at the head of the embayment, may be either established or disproven.

The next formation laid down on the map is the Port Hudson group, of which, however, the outcropping *littoral* portion only is here represented. Properly speaking, it should be shown as occupying also most of the space colored as alluvial, since it underlies everywhere, not only the marine alluvium (and a portion of the Gulf itself), but also that of the Mississippi and its chief tributaries, at least as high up as Memphis, and on Red river, nearly if not quite up to Shreveport. It seems to exist equally in the valleys of other larger rivers tributary to the Gulf; notably in that of the Pascagoula, up to one hundred miles (in a direct line) from the coast.*

Having discussed this formation somewhat in detail in papers recently published, I will merely state that it embraces a group of partly littoral and estuarian, partly swamp, lagoon and fluviatile deposits, whose thickness and location is manifestly dependent upon the topographical features of the continent, then (during the "Champlain" period) in progress of slow depression; as shown by the nature of the deposits, and the numerous superimposed generations of large cypress stumps, imbedded in laminated clays exhibiting the yearly fall of leaves. These beds overlie those of the Orange Sand or Stratified Drift, while themselves overlaid by, not only the river alluvium, but also by the Loess or Bluff silt or its equivalents; as well as where this is absent, by the Yellow Loam of the surface.

* See Miss. Rep., 1860, p. 153. The reference of the outcrop at Powe's to the Grand Gulf group is, I think, undoubtedly erroneous, and the same may be true of part or whole of the Dwyer's Ferry section, p. 154.

It would seem that here also, during the latter portion of the Drift period, most of the larger river channels were already impressed upon the surface, though not always coincident with the present immediate valley; as Newberry has observed in relation to some of the northern rivers. A depression of the land would gradually transform these channels into inlets filled with more or less stagnant fresh water down to a greater or less distance from the then existing coast line; and thus opportunity would be afforded for the formation of the swamp and lagoon deposits into which both the Mississippi and Red river have subsequently cut their channels. The banks of the Red river as well as, outside the present alluvial area, those of the many lakes and bayous which border that stream, exhibit strata absolutely identical in character with those observed near the coast, yet, of course, totally different from either the alluvial deposits of the present time, or the adjoining tertiaries. The same holds true, more or less, of the Mississippi and its mighty "bayous." According to the observations of Dr. E. A. Smith in the Yazoo bottom, and my own in that of the Tensas, not only do the clays with calcareous concretions (as characteristic of the Port Hudson age as they are foreign to the alluvium of to-day) frequently crop out in the beds of the streams; but much of the best lands of the "buckshot" kind, now situated above overflow, have clearly been formed by simple disintegration of these strata, altogether independently of the river alluvium.

These results fully confirm, therefore, the statement made by Gen. Humphreys,* that the Mississippi does not, as a rule, flow in a bed formed of its own deposits, but has excavated it in an older geological formation. Wells exceeding fifteen or twenty feet ordinarily strike these clays throughout the bottom, as they do in the delta; and the analogy has been completed by the repetition of the phenomena observed in driven wells at New Orleans,† at a point about fifty miles above Vicksburg (Gen. Wade Hampton's plantation), where a tube well has furnished a copious flow of combustible gas undiminished for many months.

The swamp clays form, however, only the lower portion of the Port Hudson beds. Higher up, as shown at the Port Hudson bluff,‡ there lie yellow or whitish silts and "hard-pans." These form, also, a level terrace some miles in width, bordering the Tensas bottom; while high above it, on the hill, tops of Sicily Island, on the Washita, lie the remnants of the Loess formation, the main body of which has succumbed to the erosive influence of the Terrace epoch of elevation. It has, however, left a belt a few miles wide on the eastern side of the valley, as shown on the map.

* Rep. on the Mississippi river, p. 98, et al. † This Jour., vol. i, 1871, p. 345.
‡ See profile in this Jour., Jan., 1869.

It would thus seem that during the latter portion of the period of depression, the rate of sinking became, at times, too rapid to allow of the accumulation of swamp deposit. The indurate silts are mostly void of fossils of any kind, but are occasionally traversed by fluviatile beds with pebbles, drift-wood, etc. Then there is a recurrence of the swamp deposits; then again silts; and finally, the calcareous, silty loam of the Bluff formation, with its numerous terrestrial fossils and "Löss puppets," ends the deposits clearly referable to the epoch of depression.

The Loess differs little from its equivalents farther north, save in being utterly devoid of stratification as well as of any fluviatile organisms. It is not easy to imagine the *modus operandi* by which a deposit of this kind, sometimes 70 feet thick, and of dead uniformity from top to bottom, could be produced. Its equivalents farther north exhibit very distinctly the structure resulting when deposition takes place in (gently) flowing water; at the south it was probably substantially stagnant, save as regards the tidal flow. Perhaps the latter may serve to explain both the absence of fluviatile as well as marine life, and the uniform intermixture, without any semblance of arrangement, of material varying from the finest silt to pebbles half an inch in diameter. A strong tidal wave running up a deep inlet of this kind, would naturally sweep away, in its periodical rushes, many members of the terrestrial fauna, whose remains are in a marked degree the more abundant the nearer we approach to the edge of the formation.

Overlying the Loess we find, wherever opportunity is afforded, a stratum of yellow loam or brick clay, which near the larger valleys is often as much as fifteen to twenty feet in thickness. It is altogether devoid of stratified structure, as well as of fossils, and forms the surface layer, and in most cases the subsoil of the Gulf States. If, as I am inclined to believe, its presence as a connected, though very undulating sheet, on all but the most elevated uplands of these States, necessitates the assumption of submergence, however brief, to the highest level at which it occurs; the changes of level heretofore alluded to would be shown to have exceeded by 600 to 700 feet the estimate given above.

The succeeding (Terrace) epoch of elevation has not, so far as I am aware, left any marks in the way of beach-lines or terraces, unless the second bottoms or "hommocks" be accounted such. They, however, belong to a very modern epoch, for they occur on streams no larger than what is usually called a "creek," and are most marked on the smaller rivers; while, apparently absent from those of the largest size, such as the Mississippi, Red and Arkansas rivers. The elevation at which,

on the very Gulf shore, we find deposits of the Port Hudson age (180 feet at the Five Islands on Vermillion Bay) shows, nevertheless, that a stupendous amount of erosion was accomplished during the time that the Mississippi occupied in scooping out its channel, to a depth which, even below the northern boundary of Louisiana, cannot be estimated at less than 500 feet.

As regards the modern epoch, I will merely remark that, while in the axis of the ancient embayment the Mississippi river, through the singular instrumentality of mudlumps upheaval, is rapidly pushing out the land into the Gulf waters, the latter are nevertheless gaining ground on almost the entire coast of Mississippi and Alabama; and the same is true of a portion of Vermillion Bay. Yet on the whole, the coast of Louisiana, as well as that of Texas and Florida, is more than holding its own; and the shallowness of the water, even where encroachment does take place, will necessarily restrict the latter within narrow limits hereafter.

ART. L.—*On the Astronomical Proof of a Resisting Medium in Space;* by ASAPH HALL.

THE return of Encke's Comet during the present year and its very favorable position for observation will attract the attention of astronomers to this, one of the most interesting bodies of our solar system. Besides the interest belonging to all periodic comets since the establishment of the probable connection of their orbits with those of meteoric streams, this comet has a peculiar interest, since from the singular anomaly in its motion Professor Encke drew his inference of a resisting medium in space. Encke's labors on the orbit of this comet were begun in 1819; and immediately after his discovery of its periodicity, he found by comparing the observations of that year with those of 1786, 1795 and 1805, and taking careful account of the planetary perturbations, the remarkable circumstance that the periodic times were diminishing. The following values of these times were found:

1786—1795	periodic time	=	1208·112 days
1795—1805	" "	=	1207·879 "
1805—1819	" "	=	1207·424 "

In order to account for this diminution Encke adopted the hypothesis of a resisting medium in space. He appears to have been led to this hypothesis in the first place on account of its inherent probability, and in this view he was sustained by

Olbers. But the strongest proof of its truth lies in the fact that the analytical investigation shows, that a tangential force resulting from a resisting medium would produce secular changes in the mean motion of the body and in the excentricity of its orbit, leaving the other elements unchanged or changed by periodic inequalities only. This conclusion is independent of the law of density of the medium. Now this was what Encke needed in order to account for the anomalous motion of the comet, the change falling almost entirely on its mean motion, that of the excentricity being quite insignificant. Other hypotheses were suggested to explain the diminution of the periodic time, and especially that of internal changes in the comet itself, but nearly all of these, besides being less simple than the assumption of a resisting medium, would necessitate the introduction of forces acting in various directions, and producing anomalous changes in all the other elements of the orbit, contrary to what was required by the observations. Encke therefore, notwithstanding the doubts of Bessel and other astronomers, continued steadfast in his theory of a resisting medium in space, and for more than forty years, and until within a short time before his death in 1865, pursued his calculations with wonderful zeal and industry. Between the years 1829 and 1859, he published in the volumes of the Berlin Academy eight memoirs on the orbit of this comet, and also other investigations on the same subject in the Astronomische Nachrichten and in the Berlin Jahrbuch. He assures us, what we can easily believe, that he spared no labor and despised no precaution that could give completeness and surety to his computations; and besides being an excellent mathematician, Encke possessed, in a degree rarely equaled, the skill of adapting formulæ to convenient and safe forms for numerical calculations. He has given in the Berlin Jahrbuch for 1861 a résumé of his labors, and the proofs presented there, taken simply by themselves, seem to put beyond the shadow of a doubt two conclusions: first, that the periodic time of this comet is diminishing; and secondly, that this diminution is satisfactorily accounted for by the assumption of a resisting medium in space.

I will now state the reasons that throw doubt on the preceding conclusions, and which, I think, require that Encke's results should be tested by an independent calculation.

The position and dimensions of the orbit of Encke's comet are such that the comet can approach very near to Mercury, so near indeed, that notwithstanding the small mass of this planet, the perturbations which it may produce in the motion of the comet can exceed the greatest ever produced by Jupiter. On account of the rapid motion of Mercury, the calculation of

these perturbations would be very laborious, and frequent corrections of the comet's elements would be necessary. It is well known that among the incidental results of Encke's investigations on the orbit of this comet, were the corrections of the values of the masses of Jupiter, Mars and Mercury given by Laplace. The value of the mass of Jupiter is now accurately known, but with regard to Mars and Mercury there is an uncertainty which of course it would be necessary to consider in any investigation where these masses can produce large disturbances. Considering, therefore, the great difficulty of the problem, it does not seem unreasonable to ask, before accepting any extraordinary assumption, that Encke's results should be verified by a new calculation, carried on at least through four or five successive revolutions of the comet during recent times, when the observations have been accurately made. Encke himself has given in the Berlin Jahrbuch for 1858 a new and rigorous method for such calculations.

But should it be found, as seems probable, that Encke's numerical results are correct, it would not follow that the existence of a resisting medium in space is established.

There are two other periodical comets whose motions have been carefully investigated, and which furnish important evidence on this question. These are Faye's and Winnecke's comets, discovered, or in the case of the last rediscovered, in 1843 and 1858, and which have periods of $7\frac{1}{2}$ and $5\frac{1}{2}$ years respectively. If we denote by q the perihelion distance of a comet, by a its semi-major axis, and by e the excentricity of its orbit, and express these quantities in units of the earth's mean distance from the sun, we shall have the following values for 1858, when all these comets were observed.

Comet.	q.	a.	e.
Encke,	0·3407	2·2181	0·8464
Faye,	1·6942	3·8137	0·5556
Winnecke,	0·7684	3.1367	0·7550

If we observe that the aphelion distance is $2a-q$, the preceding quantities will give us an idea of how differently situated in space are the orbits of these three comets, and with what different velocities they move around the sun. Thus, while Encke's comet at its perihelion approaches nearer to the sun than Mercury, and always remains nearer than Jupiter, on the other hand, Faye's comet never approaches so near the sun as does the planet Mars. Should it be found, therefore, that all these comets exhibit in their motions the anomaly found by Encke, and could this be accounted for by the assumption of a resisting medium in space, the evidence would be considered decisive. Such, however, is not the fact. The orbit of Faye's comet has

been very carefully determined by Professor Axel Moéller of Lund, Sweden. After computing the planetary perturbations, Prof. Moéller combined the positions of the comet observed in 1843, 1851 and 1858, and at first thought it necessary to introduce, as Encke had done, the hypothesis of a resisting medium. In this way he satisfied the observations of the three returns in an admirable manner, and Encke in his declining years thought he saw the complete proof of his hypothesis and a satisfactory reply to Bessel's objection made in 1835, "that up to the present time we know of nothing but the motion of a *single* comet which it is necessary to explain by a resisting medium in space, since the motions of the planets and moon have given no evidence of a resistance." But in 1865 Prof. Moéller revised his calculations, and found that he had made an error in computing the pertubations, or rather in transferring the varying elements of the comet's orbit from one epoch to another, and that when this was correctly done the observations were satisfied, within the limits of their probable error, by a strict adherence to the law of gravitation and without any extraordinary hypothesis. Therefore, laying aside the theory of a resisting medium, Prof. Moéller carried forward his calculation of the perturbations, and computed an ephemeris for the return of the comet in 1865. This prediction proved to be of wonderful accuracy. The comet was so faint in that year, that it could be seen only with difficulty in the telescope of the Naval Observatory, but on the first night it was looked for it was seen exactly in the predicted place. Professor Peters of Hamilton College, who is provided with a more powerful telescope, made a series of accurate determinations of position and from the mean of eight days' observations the corrections of Prof. Moéller's ephemeris were only

$$\Delta\alpha = +0^{s}\cdot55,\ \Delta\delta = +9''\cdot9.$$

The prediction, therefore, was one of the most accurate ever made of the return of a comet. Hence in the case of Faye's comet, the weight of evidence was transferred from one scale to the other; and the character of the error, which Prof. Moéller detected in his own work, is such that there is additional reason for desiring that Encke's calculation should be subjected to a rigorous test.

Winnecke's comet was seen first in 1819, when Encke computed an elliptic orbit which gave very nearly the true periodic time, but the comet was not seen again until it was rediscovered by Dr. Winnecke in 1858. The most careful determination of its orbit is that by Professor Oppolzer of Vienna. As the perturbations of this comet have not been large, regard was had to the first powers only of the disturbing forces, and the calculations have been made with comparative ease. By combining

the positions of 1819, 1858 and 1869, Prof. Oppolzer finds for the first interval the value of the mean motion 638″·6312, and for the second interval 638″·7007. This difference is so small that we may safely conclude that the comet's motion is strictly in obedience to the law of gravitation.

Hence, so far as the motions of comets have been determined, the evidence is against the theory of a resisting medium in space. Thus far, the observations of the planets lead to the conclusion, that their motions are in strict accord with the law of gravitation; and in the disputes about the acceleration of the mean motion of the moon, no one has thought to seek its cause in a resisting medium, but much more probable causes are at hand. Encke's comet, therefore, stands alone in the strange anomaly in its motion which the calculations have shown. The first thing to be done would be to test the correctness of these calculations; and for this purpose it seems to me that the method of special perturbations is better than the expansion of the general perturbations, since by the first method all powers of the disturbing forces can be rigorously taken account of, while in the method of general perturbations the theory is difficult, and in the present state of analysis a doubt may remain whether we have included in our result all terms of a sensible influence. If it be proved that the diminution of the periodic time actually exists, this anomaly must be considered as a peculiarity of Encke's comet, and its cause must be sought for in something which distinguishes this comet from all others. It was early pointed out, by Olbers I think, that this comet moves through those regions where the zodiacal light is seen. Possibly also the numerous meteoric streams which are moving around the sun, and which are closely connected with the orbits of some of the comets, may exert an influence on their motions.

Sept. 25, 1871.

ART. LI.—*On a new Micrometric Goniometer eye-piece for the Microscope;* by J. P. SOUTHWORTH.

AFTER a few experiments by Dr. H. T. Porter and myself, we have succeeded in making an eye-piece micrometer and goniometer which equal in accuracy and surpass in simplicity and cheapness any we have seen, and we have used those of some of the best makers in this country. The objection to the eye-piece micrometers in use is the want of boldness in the division-lines, which makes them fatiguing and hurtful to the eyes. To overcome this objection we were led to experiments in making micrometers by the aid of photography, which have resulted in success. The steps of the process are these:—

1st. A scale of 100 heavy India ink lines about $\frac{1}{6}$ of an inch apart are drawn on a dead white surface of Bristol board. The lines marking every ten divisions are six inches long and extend one inch each side of the scale; those marking every five divisions are five inches long and extend one half inch beyond the scale; the remaining lines are four inches long.

2d. By photographic process for copying engravings, a negative is taken, on which the scale equals about two inches in length, and is intensified by mercuric chloride and potassium cyanide.

3d. With a copying camera and lens for taking transparent positives for the magic lantern, a transparent positive of this negative is taken on micrometer glass, reducing the scale to the length of one-half inch. In this the lines are $\frac{1}{2\,0\,0}$ of an inch apart. After intensifying, washing and drying, a cover of thin glass is cemented on with Canada balsam, and the slide cut to fit the slit in the micrometer eye-piece. It can be also mounted with a spring and micrometer screw, like Jackson's micrometer. In our micrometer the lines appear to stand out in relief, and are jet black, while the spaces between them are translucent enough to admit of the accurate measurement of the details of minute algæ and fungi to the $\frac{1}{2\,5\,0\,0\,0}$ of an inch.

Regarding the goniometer:—

1st. A circle about eighteen inches in diameter is drawn with India ink, divided into degrees. The center is indicated by a dot, and one diameter is drawn. Every five and ten degrees are indicated by longer lines than those indicating single degrees. Every ten degrees of each quadrant are numbered from 0 to 90.

2d. A negative two inches in diameter is taken by the process referred to above, and from this a transparent positive is taken on a circle of micrometer glass cut to fit the tube of the microscope. It is covered with a circle of thin glass cemented with balsam, and mounted to fit the tube at the focal point of a positive eye-piece. A cobweb is drawn across the diameter of the lower lens. When a crystal is to be measured, the stage is moved till the apex of the angle coincides with the center of the goniometer and the diameter with one side. The eye-piece is now turned till the cobweb crossing the diameter at the center coincides with the other side of the angle. Now the number of degrees of the angle can be read at the circumference. The advantage of this over the ordinary microscopic goniometers is that in ours the angles of the crystal and the degrees of the goniometer are on the same line of sight within the tube of the microscope, while in the ordinary goniometer the degrees are marked outside the tube. The photographic processes by which the above are made can be learned by consulting any of the standard works on photography, under the sections that treat of copying engravings and taking transparent positives.

Georgetown, D. C.

ART. LII.—*On the bearing of Devonian Botany on Questions as to the Origin and Extinction of Species;* by Dr. J. W. DAWSON.

[The theoretical views contained in this section, though necessary to give completeness to the subject, are not suitable for an official report, and are, therefore, printed separately by the author, for circulation to those who may be interested in them as matters of science.]

FOSSIL plants are almost proverbially uncertain with reference to their accurate determination, and have been regarded as of comparatively little utility in the decision of general questions of paleontology. This results principally from the fragmentary condition in which they have been studied, and from the fact that fragments of animal structures are more definite and instructive than corresponding portions of plants.

It is to be observed, however, that our knowledge of fossil plants becomes accurate in proportion to the extent to which we can carry the study of specimens in the beds in which they are preserved, so as to examine more perfect examples than those usually to be found in museums. When structure is taken into the account, as well as external form, we can also depend more confidently on our results. Further, the abundance of specimens to be obtained in particular beds often goes far to make up for their individual imperfection. The writer of these pages has been enabled to avail himself very fully of these advantages, and on this account, if on no other, feels entitled to speak with some authority on theoretical questions.

It is an additional encouragement to pursue the subject that, when we can obtain definite information as to the successive floras of any region, we thereby learn much as to climate, and vicissitudes in regard to the extent of land and water; and that, with reference to such points, the evidence of fossil plants, when properly studied, is, from the close relation of plants to those stations and climates, even more valuable than that of animal fossils.

It is necessary, however, that in pursuing such enquiries we should have some definite views as to the nature and permanence of specific forms, whether with reference to a single geological period, or to successive periods; and I may be excused for stating here some general principles, which I think important for our guidance, with special reference to the paleozoic floras which form the subject of this memoir.

(1.) Botanists proceed on the assumption, vindicated by experience, that, within the period of human observation, species have not materially varied or passed into each other. We may make, for practical purposes, the same assumption with regard

to any given geological period, and may hold that for each such period there are specific types, which, for the time at least, are invariable.

(2.) When we inquire what constitutes a good species for any given period, we have reason to believe that many names in our lists represent merely varietal forms or erroneous determinations. This is the case even in the modern flora; and in fossil floras, through the poverty of specimens, their fragmentary condition and various states of preservation, it is still more likely to occur. Every revision of any group of fossils detects numerous synonyms, and of these many are incapable of detection without the comparison of large suites of specimens.

(3.) We may select from the flora of any geological period certain forms, which I shall call *specific types*, which may for such period be regarded as unchanging. Having settled such types, we may compare them with similar forms in other periods, and such comparisons will not be vitiated by the uncertainty which arises from the comparison of so-called species which may, in many cases, be mere varietal forms, as distinguished from specific types. Our types may be founded on mere fragments, provided that these are of such a nature as to prove that they belong to distinct forms which cannot pass into each other, at least within the limits of one geological period.

(4.) When we compare the specific types of one period with those of another immediately precedent or subsequent, we shall find that some continue unchanged through long intervals of geological time, that others are represented by allied forms regarded either as varietal or specific, and as derived or otherwise, according to the view which we may entertain as to the permanence of species. On the other hand, we also find new types not rationally deducible, on any theory of derivation, from those known in other periods. Further, in comparing the types of a poor period with those of one rich in species, we may account for the appearance of new types in the latter by the deficiency of our information as to the former; where many new types appear in the poorer period this conclusion seems less probable. For example, new types appearing in poor formations, like the Lower Erian and Lower Carboniferous, have greater significance than if they appeared in the Middle Erian or in the Coal Measures.

(5.) When specific types disappear without any known successors, under circumstances in which it seems unlikely that we should have failed to discover their continuance, we may fairly assume that they have become extinct, at least locally; and where the field of observation is very extensive, as in the great coal fields of Europe and America, we may esteem such extinction as practically general, at least for the northern hemi-

sphere. When many specific types become extinct together, or in close succession, we may suppose that such extinction resulted from physical changes; but where single types disappear, under circumstances in which others of similar habit continue, we may not unreasonably conjecture that, as Pictet has argued in the case of animals, such types may have been in their own nature limited in duration, and may have died out without any external cause.

(6.) With regard to the *introduction* of specific types we have not as yet a sufficient amount of information. Even if we freely admit that ordinary specific forms, as well as mere varieties, may result from derivation, this by no means excludes the idea of primitive specific types originating in some other way. Just as the chemist, after analyzing all compounds and ascertaining all allotropic forms, arrives at length at certain elements not mutually transmutable or derivable, so the botanist and zoölogist must expect sooner or later to arrive at elementary specific types, which, if to be accounted for at all, must be explained on some principle distinct from that of derivation. The position of many modern biologists, in presence of this question, may be logically the same with that of the ancient alchemists with reference to the chemical elements, though the fallacy in the case of fossils may be of more difficult detection. Our business at present, in the prosecution of paleobotany, is to discover, if possible, what are elementary or original types, and, having found these, to enquire as to the law of their creation.

(7.) In prosecuting such questions geographical relations must be carefully considered. When the floras of two successive periods have existed in the same region, and under circumstances that render it probable that plants have continued to grow on the same or adjoining areas throughout these periods, the comparison becomes direct, and this is the case with the Erian and Carboniferous floras in North-Eastern America. But when the areas of the two formations are widely separated in space, as well as in time, any resemblances of facies that we may observe may have no connection whatever with an unbroken continuity of specific types.

I desire, however, under this head, to affirm my conviction that, with reference to the Erian and Carboniferous floras of North America and of Europe, the doctrine of "homotaxis," as distinct from actual contemporaneity, has no place. The succession of formations in the Paleozoic period evidences a similar series of physical phenomena on the grandest scale throughout the northern hemisphere. The succession of marine animals implies the continuity of the sea-bottoms on which they lived. The head-quarters of the Erian flora in America and Europe must have been in connected or adjoining areas in the North

Atlantic. The similarity of the Carboniferous flora on the two sides of the Atlantic, and the great number of identical species, proves a still closer connection in that period. These coincidences are too extensive and too frequently repeated to be the result of any accident of similar sequence at different times, and this more especially as they extend to the more minute differences in the features of each period, as, for instance, the floras of the Lower and Upper Devonian, and of the Lower, Middle, and Upper Carboniferous.

Another geographical question is that which relates to centers of dispersion. In times of slow subsidence of extensive areas, the plants inhabiting such areas must be narrowed in their range and often separated from each other in detached spots, while, at the same time, important climatal changes must also occur. On the re-emergence of the land, such of these species as remained would again extend themselves over their former areas of distribution, in so far as the new climatal and other conditions would permit. We should naturally suppose that the first of the above processes would tend to the elimination of varieties, the second to their increase; but, on the other hand, the breaking up of a continental flora into that of distinct islets, and the crowding together of many forms, might be a process fertile in the production of some varieties, if fatal to others.

Further, it is possible that these changes of subsidence may have some connection with the introduction, as well as with the extinction, even of specific types. It is certain, at least, in the case of land plants, that such types come in most abundantly immediately after elevation, though they are most abundantly preserved in periods of slow subsidence. I do not mean, however, that this connection is one of cause and effect; there are, indeed, indications that it is not so. One of these is, that in some cases the enlargement of the area of the land seems to be as injurious to terrestrial species as its diminution.

Applying the above considerations to the Erian and Carboniferous floras of North America, we obtain some data which may guide us in arriving at general conclusions. The Erian flora is comparatively poor, and its types are in the main similar to those of the Carboniferous. Of these types a few only re-appear in the Middle Coal formation under identical forms; a great number appear under allied forms; some altogether disappear. The Erian flora of New Brunswick and Maine occurs side by side with the Carboniferous of the same region; so does the Erian of New York and Pennsylvania with the Carboniferous of those States. Thus we have data for the comparison of successive floras in the same region. In the Canadian region we have, indeed, in direct sequence, the floras of the Upper Silurian, the Lower, Middle, and Upper Erian, and the Lower,

Middle, and Upper Carboniferous, all more or less distinct from each other, and affording an admirable series for comparison in a region whose geographical features are very broadly marked. All these floras are composed in great part of similar types, and probably do not indicate very dissimilar general physical conditions, but they are separated from each other by the great subsidences of the Corniferous limestone and the Lower Carboniferous limestone, and by the local but intense subterranean action which has altered and disturbed the Erian beds toward the close of that period. Still none of these changes was universal. The Corniferous limestone is absent in Gaspé, and probably in New Brunswick, where, consequently, the Erian flora could continue undisturbed during that long period. The Carboniferous limestone is absent from the slopes of the Appalachians in Pennsylvania, where a retreat may have been afforded to the Upper Erian and Lower Carboniferous floras. The disturbances at the close of the Erian were limited to those eastern regions where the great limestone-producing subsidences were unfelt, and, on the other hand, are absent in Ohio, where the subsidences and marine conditions were almost at a maximum.

Bearing in mind these peculiarities of the area in question, we may now group in a tabular form the distinct specific types recognized in the Erian system, indicating, at the same time, those which are represented by identical species in the Carboniferous, those represented by similar species of the same general type, and those not represented at all. For example, *Calamites cannæformis* extends as a species into the Carboniferous; *Asterophyllites latifolius* does not so extend, but is represented by closely allied species of the same type; *Prototaxites* disappears altogether before we reach the Carboniferous.

Of the accompanying forms, fifty-one in all, found in the Erian of Eastern America, all, except the four last, are certainly distinct specific types. Of these only four reappear in the Carboniferous under identical species, but no less than twenty-six reappear under representative or allied forms, some at least of which a derivationist might claim as modified descendants. On the other hand, nearly one half of the Devonian types are unknown in the Carboniferous, while there remain a very large number of Carboniferous types not accounted for by anything known in the Devonian. Further, a very poor flora, including only two or three types, is the predecessor of the Erian flora in the Upper Silurian, and the flora again becomes poor in the Upper Devonian and Lower Carboniferous. Every new species discovered must more or less modify the above statements, and the whole Erian flora of America, as well as the Carboniferous, requires a thorough comparison with that of Europe before general conclusions can be safely drawn. In the meantime I may indicate the

Table of Erian and Carboniferous Specific Types.

Erian Types. Represented in Carboniferous—	by identical types.	by relat'd forms.	Erian Types. Represented in Carboniferous—	by identical types.	by relat'd forms.
1. Syringoxylon mirabile,			27. Cordaites Robbii,		*
2. Nematoxylon,			28. C. augustifolia,		
3. Prototaxites,			29. Cyclopteris (Archæopteris),		
4. Aporoxylon,			30. C. (Aneimites),		*
5. Ormoxylon,			31. C. Brownii,		
6. Dadoxylon,		*	32. C. varia,		*
7. Sigillaria Vanuxemii,		*	33. Neuropteris polymorpha,		*
8. S. palpebra,		*	34. N. Serrulata,		*
9. Didymophyllum,			35. N. Dawsonii,		
10. Calamodendron,		*	36. N. retorquata,		*
11. Calamites transitionis,	*		37. N. resecta,		
12. C. cannæformis,	*		38. Spenopteris Hœninghausi,	*	
13. Asterophyllites scutigera,			39. S. Harttii,		*
14. A. latifolia,		*	40. Hymenophyllites curtilobus,		
15. Annularia laxa,			41. H. obtusilobus,		*
16. Sphenophyllum antiquam,		*	42. Alethopteris discrepans,		*
17. Cyclostigma,			43. Pecopteris serrulata,		*
18. Arthrostigma,			44. P. preciosa,		
19. Lepidodendron Gaspianum,		*	45. Trichomanites,		*
20. L. Veltheimianum,		*	46. Callipteris,		*
21. Lycopodites Matthewi,		*	47. Psaronius,		*
22. L. Richardsoni,			48. Cardiocarpum,		*
23. L. Vanuxemii,			49. C. Crampii,		
24. Lepidophloios antiquus,		*	50. Antholithes,		*
25. Psilophyton princeps,			51. Trigonocarpum.		*
26. P. robustius,					

direction in which the facts seem to point, by the following general statements:—

1. Some of the forms reckoned as specific in the Devonian and Carboniferous may be really derivative races. There are indications that such races may have originated in one or more of the following ways:—(1) By a natural tendency in synthetic types to become specialized in the direction of one or other of their constituent elements. In this way such plants as *Arthrostigma* and *Psilophyton* may have assumed new varietal forms. (2) By embryonic retardation or acceleration,† whereby certain species may have had their maturity advanced or postponed, thus giving them various grades of perfection in reproduction and complexity of structure. The fact that so many Erian and Carboniferous plants seem to be on the confines of our groups of Acrogens and Gymnosperms may be supposed favorable to such exchanges. (3) The contraction and breaking up of floras, as occurred in the Middle Erian and Lower Carboniferous, may have been eminently favorable to the production of such varietal forms as would result from what has been called the "struggle for existence." (4) The elevation of a great expanse of new land at the close of the Middle Erian and the beginning

† In the manner illustrated by Hyatt and Cope.

of the Coal period, would, by permitting the extension of species over wide areas and fertile soils, and by removing the pressure previously existing, be eminently favorable to the production of new, and especially of improved, varieties.

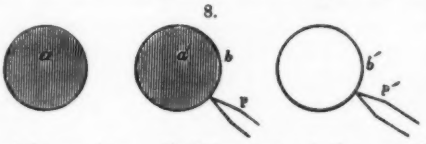
2. Whatever importance we may attach to the above supposed causes of change, we still require to account for the origin of our specific types. This may forever elude our observation, but we may at least hope to ascertain the external conditions favorable to their production. In order to attain even to this it will be necessary to inquire critically, with reference to every acknowledged species, what its claims to distinctness are, so that we may be enabled to distinguish specific types from mere varieties. Having attained to some certainty in this, we may be prepared to inquire whether the conditions favorable to the appearance of new varieties were also those favorable to the creation of new types, or the reverse—whether these conditions were those of compression or expansion, or to what extent the appearance of new types may be independent of any external conditions, other than those absolutely necessary for their existence. I am not without hope that the further study of fossil plants may enable us thus to approach to a comprehension of the laws of the creation, as distinguished from those of the continued existence of species.

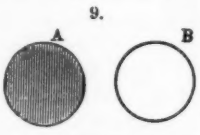
In the present state of our knowledge we have no good ground either to limit the number of specific types beyond what a fair study of our material may warrant, or to infer that such primitive types must necessarily have been of low grade, or that progress in varietal forms has always been upward. The occurrence of such an advanced and specialized type as that of *Syringoxylon*, in the Middle Devonian, should guard us against these errors. The creative process may have been applicable to the highest as well as to the lowest forms, and subsequent deviations must have included degradation as well as elevation. I can conceive nothing more unreasonable than the statement sometimes made that it is illogical or even absurd to suppose that highly organized beings could have been produced except by derivation from previously existing organisms. This is begging the whole question at issue, depriving science of a noble department of inquiry on which it has as yet barely entered, and anticipating by unwarranted assertions conclusions which may perhaps suddenly dawn upon us through the inspiration of some great intellect, or may for generations to come baffle the united exertions of all the earnest promoters of natural science. Our present attitude should not be that of dogmatists, but that of patient workers content to labor for a harvest of grand generalizations which may not come till we have passed away, but which, if we are earnest and true to nature and its Creator, may reward even some of us.

ART. LIII.—*On some Phenomena of Binocular Vision;* by JOSEPH LECONTE, Prof. Geol. and Nat. Hist., University of California.

In giving my own explanation of the phenomena of M. Pictet's experiments, I will commence with the experiment with the piece of money. If M. Pictet had made this experiment *without* the median screen, it seems to me the true explanation could not have escaped him. Let us then first try it without the screen.

If I place a piece of money on a sheet of paper lying on the table, and look downward in the direction of the piece, but at the same time gaze on vacancy, I see two heteronymous images, $a\,a'$ (fig. 8), separated by an interocular space. If I now attempt to outline these images, I see also *two images of the pencil.* If I use the *right-eye image* (left image) of the pencil p to draw the *left-eye image* (right image) of the money a', then I see one pencil tracing the outline b of the image a', while another pencil makes a tracing b' with no money in it. If I now examine the result of this experiment, I find the tracing I have made, B (fig. 9), at some distance (an interocular space) from the piece A to the right. The explanation is obvious. In gazing on vacancy, as already explained (2), the whole field of view is shifted by each eye heteronymously a half interocular space. The left-eye image of A (fig. 9) and the right-eye image of the spot B are brought together and superposed ($a'\,b$, fig. 8); while the right-eye image of A and the left-eye image of B are seen to the left and right respectively (a and b', fig. 8). It is precisely the same as the superposition of the double images of two fingers described on page 165. If, instead of using the right image of the pencil to draw the left-eye image of the money, I use *corresponding images* of the pencil and money, i. e., right-eye images or left-eye images of both, I find *I place the pencil on the money.* Finally, if I use the left-eye image p', fig. 8, of the pencil to draw the right-eye image a of the money, I find I have made a tracing *an interocular distance to the left,* and the result of both experiments is two tracings a double interocular distance apart.





Now in M. Pictet's mode of performing the experiment, by the use of the median screen, we cut off the right-eye image of the money a, fig. 8, and the left-eye image of the pencil p', fig. 8, and we therefore see only the left-eye image of the money a'

and the right-eye image of the pencil *p;* and these, being in the visual line of the two eyes, are brought together by the law of corresponding points (2) precisely as the two pictures of a stereoscopic card are united, or as any two objects, an interocular space apart, are superposed when we gaze at a distant point. If M. Pictet had used his *left hand to draw*, then he *would have used corresponding images* of the pencil and piece; and he would have found that in attempting to draw his *illusive* image he *would have placed his pencil on the piece.*

10.

11.

In the experiment *without* the median screen, fig. 10 gives the actual relation of parts, and fig. 11 represents, by my method, the visual result. In the experiment *with* the median screen, the same are represented by figs. 12 and 13. Comparing the visual results, figs. 11 and 13, it will be observed that the additional images, viz: *a* and P′, are cut off by the median screen.

12.

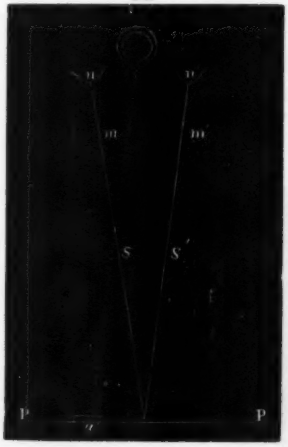
13.

It is evident, therefore, that in all M. Pictet's experiments, the image we see and trace in outline is not an *image of illusion* seen by the right, but a *real image* seen by the left, eye. The pencil we see with the right eye, and the two points, viz: the pencil and money, or the part of the paper on which we make the drawing and the money, being in the visual lines, are brought

together by the law of corresponding points. In M. Pictet's experiment these two, the pencil and the money, are similarly related to the two eyes, one on one side and the other on the other side of the screen—one exposed to the view of one eye and the other to the view of the other eye. If the image we draw is an illusive image seen by the right eye, then the *pencil* with which we draw must be also an illusive image seen by the left eye.

But to explain M. Pictet's experiment a little farther: When we look *directly at the money*, M. Pictet says "we see that the vertical screen is transparent throughout, and that it permits the right eye to see the piece as through a perfectly diaphanous surface." But there are *two* transparent screens seen. The one seen by the right eye M. Pictet observes,* the other apparently escapes his observation. The truth is, when we look at the money, the heteronymously doubled images of the median screen *m m'* (fig. 14) meet at the distance of the point of sight. The actual relation of parts is seen in fig. 7 (p. 322), in which A R and A L are the visual lines converged upon the piece A. The visual result is seen in fig. 14. It is seen that the visual line of the right eye stops at the right eye image of the median screen, while the left visual line runs parallel to its image of the median screen unobstructed to the piece *a'*. Again, "if we give to the optic axes a direction more parallel," says M. Pictet, "we see the image of illusion move gradually to the right, traverse the screen, and come on the right side." But again, he does not observe that there are two screens seen; and again, it is the left eye image of the screen which he neglects. In truth, as the eyes become parallel, the two images of the screen, *m* S and *m'* S' fig. 14, gradually open until they become parallel, and the piece is seen between them, as already shown in fig. 13. The *piece does not in the least change its position in relation to the screen seen by the left eye;* only the right eye *shifts its image of the screen* to the left of it. If M. Pictet would place another piece of money on the right side of the screen exactly where he made the outline tracing, he would observe the two pieces unite in one, precisely as stereoscopic pictures are united. According to M. Pictet's principles, this must be regarded as the *union of two illusive images.* Where, then, are the real images?

14.



* There seems to be a kind of *dexterity* in the right eye. In many cases of double images, most persons habitually neglect the left-eye image.

It is now easily understood that by the use of lenses before the *right eye*, in M. Pictet's experiments, the image is not affected, not because it is *illusive*, but because it is the image of the *left eye*. But if the experiment be made *without* the median screen, then the true right-eye image *a*, fig. 8, will be seen to the left and *will* be enlarged. The explanation of the outlining of objects seen under the microscope is, of course, precisely the same, as is also that of the tracing on the blank half of a stereoscopic card the outline of a picture existing on the other half.

I might illustrate the subject farther in many ways, but it seems scarcely necessary. I will only remark, in passing, that by the movement of the fields of view already explained (3), it is easy, by voluntary squinting, to outline a piece of money, *in any part* of a sheet of paper one may desire. I now place a piece of money on a sheet of paper lying on the table. I place the pencil on any point where I desire to make the outline; it may be 4, 6, 10 or 12 inches from the piece. By squinting, I now bring together the right-eye image of the piece and the left-eye image of the pencil, and then trace the outline. It is a little difficult, it is true, without some small object at the point of optic convergence (point of sight) to hold the axes steady, and, therefore, to make the tracing accurate. I only speak of it to illustrate the principle of making tracings of objects at any distance from the object itself. In the case of squinting, of course a median screen is inadmissible.

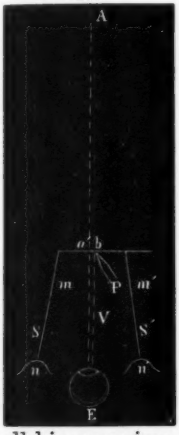
The phenomena of M. Pictet's first experiment, fig. 6, will now be easily understood. If *no median* screen is used, then fig. 15 will represent the actual relation of parts, and fig. 16 the visual result. By comparing these two figures, it will be seen that the two visual lines *v v* are brought together, so that the left-eye image of A, and the right-eye images of the spot *b* and the pencil P, fig. 15, are brought in the same line in fig. 16; but the left-eye images of the spot *b* and of the pencil

15.

16.

P, fig. 15, are seen to the right b' P', fig. 16. If we attempt to use P' instead of P to make the outline, we would miss the screen. Now in M. Pictet's experiment *with* the median screen fig. 6, this screen cuts off the left-eye image of the spot b and of the pencil P, so that we have only the left-eye image of the object A, and the right-eye image of the spot b; and these, by the law of corresponding points, are brought in the same line. The visual result is represented in fig. 17.

17.

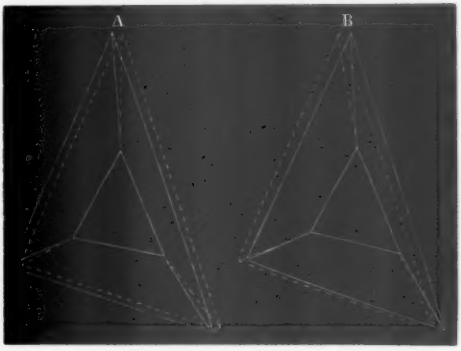


As illustrating the singular confusion into which M. Pictet has fallen, I would draw attention to the fact, that in his experiments on the transparency of double images, described on page 8, as well as in the experiment last explained, the *double images* are regarded as *real*, while the *objects*, seen beyond as through a transparency, are regarded as *illusive*. But in the experiment with the piece of money, it is one of the *double images* which he regards as illusive.

It is unnecessary to follow M. Pictet through all his experiments, as the same principles of explanation apply to all. There is one more point, however, which I wish to take up at some length. It is the *theory of binocular relief*. I wish to show the fallacy of M. Pictet's views, and at the same time to bring out more distinctly than I have ever yet done my own views on this important subject.

M. Pictet believes that, in every act of binocular vision, there are *four external images* formed; that *each eye* has its own *real* image, produced by the luminous retinal image, and its *illusive* image, propagated from the luminous impression of the other eye, and identical with the real image of the latter; that, as the two real images differ slightly from each other, being taken from different points of view, so do their fac-similes the two illusive images; also, necessarily, that the real and illusive images of each eye differ precisely, as do the two real images or the two illusive images. He believes that *the perception of relief is the result of comparison by each eye of its real with its illusive image.* Now what advantage this theory has over the usual and simpler one of Brewster, Prevost and Brücke, considering the fact that the real and illusive image of each eye differ precisely as do the real images of the two eyes, it is impossible to imagine. But M. Pictet regards the existence of the four images not as a question of advantage, but as a question of fact. "A very simple geometric construction shows us thus four images identical, two to two." I reproduce M. Pictet's figure illustrating

this point. The full-lined figs. A and B are two projections of a truncated pyramid, as seen by the left and right eye respectively.



On the same smaller or upper base, two dotted-lined figures are drawn in such wise, that each dotted-lined figure is exactly similar to the other full-lined figure. These four figures, according to M. Pictet, represent accurately the *four images* formed in looking at a *single* truncated pyramid, the full-lined figures being the true and the dotted-lined figures the illusive images. For, says he, "if we unite in one single image these four contours (by means of a stereoscope), we experience instantly the impression of a solid body; and we see that in fact the dotted lines are covered by the full lines in the binocular image, *which confirms our deduction*" (p. 141).

Now, according to the usual theory of binocular relief, viz., that of Brücke, Prevost and Brewster, how could it be otherwise? By construction, each dotted-lined figure is the fac-simile in form of the other full-lined figure, so that when their common faces, the small triangles, are united, the full-lines of the one figure *must* coincide with the dotted lines of the other. M. Pictet has therefore, by his dotted lines, only represented in *each* of his figures what must take place in the *binocular combination of his two full-lined figures*, if there were no dotted lines present; viz., the doubling of the lines of the larger triangles or lower bases, when the small triangles or upper bases are perfectly united; a fact well known to every accurate observer, and which forms the foundation of Brücke's theory.

Brücke, Prevost and Brewster explain the perception of relief by rapid changes of optic convergence, by means of which different parts of the two dissimilar images of the same object, or of two stereoscopic pictures, are successively united. In M. Pictet's full-lined figures, for example, when the smaller triangles are united, the lines of the larger triangles are slightly doubled; and when, by less optic convergence, the larger triangles are perfectly united, then the smaller triangles are doubled. Thus, the alternately greater and less convergence, necessary to unite successively different parts of the pictures—the ranging of the point of sight back and forth—precisely

like that which takes place, in natural vision, in looking successively at nearer and more distant objects, or nearer and more distant parts of the same object, gives a distinct perception of relief.

No one who has carefully analyzed his visual impressions, either in natural vision or in stereoscopic combination of pictures, can for a moment doubt that there is in all cases *a change of optic convergence* necessary to unite the different portions of a stereoscopic picture or of a natural solid object; and that Wheatstone's idea of a *complete mental combination* of dissimilar images, though still supported by great names, is certainly erroneous. In my own case, the doubling of objects in the foreground of stereoscopic pictures, while I look at the background and *vice versa*, is as plain as any other phenomenon of vision. It is impossible for me to doubt what I see so plainly. Precisely similar phenomena I detect with ease in viewing solid objects. Brücke and Prevost are therefore certainly *right* in insisting, against Wheatstone, on the *impossibility of complete union* of all parts of a stereoscopic picture of an object at the same moment; but they are, I believe, as certainly *wrong* in insisting *on changes of optic convergence* as absolutely *necessary to the perception of relief.* It is possible to perceive relief—even *while looking steadily at one point* in a stereoscopic or in a natural scene. Dove's much quoted experiment seems to prove that we can distinctly perceive stereoscopic relief by the light of an electric spark, which according to Wheatstone's celebrated experiment lasts only $\frac{1}{224000}$ of a second; a time too short to allow *change* of optic convergence. The relative distance of objects, I think, without doubt, can be distinctly perceived by the light of a flash of lightning, which according to Arago lasts less than $\frac{1}{1000}$,* and according to Rood $\frac{1}{3000}$† of a second. These facts seem to prove that stereoscopic relief can be perceived *instantly* and *without change of optic convergence.*

This point is evidently one of capital importance in the theory of binocular vision. The *instantaneous* perception of relief is evidently *fatal to Brücke's theory.* With the assistance of my brother, Prof. John LeConte, I have, therefore, recently made a series of experiments to test its correctness. These experiments entirely confirm Dove's results, and establish beyond doubt the instantaneous perception of relief.

The apparatus used in these experiments was a first-class Ritchie's induction coil, capable of producing with ease 12-inch sparks. The contact-breaker was of such kind that the rapidity of the sparks was completely under the control of

* Arago, Œuvres complete, tome 4, p. 70. † This Jour., III, vol. i, p. 15.

the operator. A Leyden jar was introduced into the circuit in order to increase the brilliancy of the sparks. The sparks were 1–2 inches in length. I selected stereoscopic pictures in which all *other* forms of perspective were entirely wanting, so that *no relief was visible with one eye.* Outline geometrical figures are best for this purpose.

I first viewed these in the stereoscope by the continuous light of a rapid succession of sparks, until the stereoscopic combination was perfect. On making the sparks separately at long intervals, the *relief was still perfectly clear* and unmistakable. On shutting one eye, the sparks continuing at intervals, the relief disappeared; on re-opening, it immediately re-appeared.

I next tried combination of the pictures with *the naked eye by squinting.* This method is entirely removed from any suspicion of fallacy arising from any other kind of perspective; since, as already stated in my previous paper,* the binocular perspective is *inverted*, and therefore, must overbear all other forms of perspective where these exist. By a rapid succession of sparks, the combination was easy and the inverse perspective perfect. When the sparks were made with long intervals between, *still the relief was clear* and unmistakable. Shutting one eye the relief disappeared, but immediately reappeared on re-opening.

When I first commenced my experiments by either of these methods, but especially the last, a rapid succession of sparks was necessary to effect combination. After the proper axial adjustment was once obtained, it could be retained without difficulty in the interval of darkness. After some practice however, the rapid succession of sparks was no longer necessary. The combination was effected, and the relief perceived by *separate flashes alone.*

Lastly, I tried natural vision. Two small objects, (brass balls mounted on wires) were placed one beyond the other at the distance of five or six feet, and separated from each by a space of about 1 foot; sometimes in the median line, and sometimes one of them a little out of the median line, but in all cases so arranged *that their relative distance could not be detected by monocular vision*, even in the full light of day. By the spark their relative distance was *at once detectable with two eyes*, though *not with one.* This last experiment was varied in many ways, but always with the same result.

Stereoscopic combination by squinting requires considerable practice, even in the full light of day, and of course much more by the electric spark. All the *other* experiments were repeated by my brother, and my results confirmed.

* III, vol. ii, p. 1.

M. Pictet *rightly* urges Dove's experiment on the instantaneous perception of relief as fatal to Brücke's theory, but *wrongly* as confirming his own. The objection applies equally to both. In both cases there is a judgment formed from a comparison of dissimilar images or pictures, and it can make no difference whether the images are real or illusive, or whether one be real and one illusive, since the illusive image behaves in all respects and under all circumstances, precisely like the real image of the other eye.

The only *true* explanation of the instantaneous perception of relief is, I believe, that given in my paper recently published.* As already stated (3), all objects or points, either beyond or on this side the point of sight, are doubled, but differently, the former *homonymously* the latter *heteronymously*; the double images of the former are united by *less*, the latter by *greater convergence*. Now the observer *knows instinctively and without trial*, in any case of double images, *whether they will be united by greater or less convergence;* and therefore, never makes a mistake nor attempts to unite by a wrong movement of the optic axis. In other words *the eye instinctively distinguishes between homonymous and heteronymous images, referring the former to objects or points beyond, and the latter to objects or points on this side, the point of sight.* My own theory of stereoscopic relief, then, is this: The eye perceives relief *instantly*, by means of double images, as just explained; but the perception is made much clearer by *changes* of optic convergences, by ranging the eyes back and forth from foreground to background and *vice versa*, and the successive combination of different parts of the object or pictures, as maintained by Brücke.

In regard to the relative merits of the nativistic and empiristic theories, i. e., whether corresponding points are such congenitally or become so by experience, I quite agree with Donders, that there is truth in both views. In a letter to Prof. Tyndall, published in the Phil. Mag. for April, 1871, referring to the question whether the "*law of direction*" was native or acquired, I have said that instinct is nothing but "*inherited experience.*"† Precisely the same remark applies to the law of corresponding points. *It is acquired by the experience of successive generations transmitted by the law of inheritance, and made more perfect by individual experience.* The inherited experience is greater in the lower animals, the individual experience is greater in man. Binocular single vision is therefore, to a large extent, instinctive even in man, and much more so in lower animals. Doubtless, this is equivalent to saying that there is some *structural arrangement* in the nervous centers which deter-

* This Jour., III, vol. ii, p. 1, et seq.

† I had not then seen the similar view of Hering, viz., that instinct is "inherited memory."

mines single vision by corresponding points; but whether there is any such *fusion* of corresponding fibres as supposed by Müller, or any such reflection of illusive images from eye to eye as supposed by M. Pictet, can never by determined except by anatomical investigation; and even if so determined in the affirmative, could not possibly show itself in any visual phenomenon, since by supposition every such *illusive* image must be absolutely identical with, and absolutely inseparable from, a *real* image seen by the other eye.

Oakland, California, June 9, 1871.

ART. LIV.— *The American Spongilla, a craspedote, flagellate Infusorian;* by H. JAMES-CLARK, A. B., B. S., Prof. Nat. Hist., Kentucky University, Lexington, Ky. (With a Plate).

THE argument of Hæckel, and others, that the Sponges are essentially compound Polypi, is virtually based upon the assumption that the minor (afferent) and major (efferent) ostioles of the former correspond to the mouths of the latter; and that the profusely branching afferent and efferent canals of the Sponges are strictly comparable with similar canals in the polypidom of Halcyonarians: and, by implication, that the cilia-bearing cells of the interior, lining-wall of the Zoöphyte find their homologues in the ciliated, cell-like bodies of the interior chambers of the Porifera. If, now, it should turn out that these last are not altogether mere cell-components of a tissue, but are each, severally, an independent body, although closely connected with others in a common bond, then the attempted parallelism between the two groups must utterly fail of confirmation. The tendency of Carter's later investigations, and our own too, is to show that this is no vain supposition.

For ourselves, we hold that each *ciliated body* of the Sponge is a *cephalic* member (a *cephalid* in this case) of a polycephalic individual.* We believe, as far as we can understand his undecided, rather hesitating position, Carter's latest decision is, that the Sponge is a community of Amœbous individuals,† and not a polycephalic unit. Yet, whichever view prevails, the tendency is the same, and the Polyp theory is negatived most unquestionably. The incompatibility of the interior organisms of the two groups, above mentioned, is so great that it would seem as idle to elaborate a proof of it, as to attempt the demonstration of an axiom. The question is really circumscribed,

* See our article on "*Polarity and Polycephalism*," this Journal, January, 1870.

† See Carter, *On Fecundation in the two Volvoces; on Eudorina, Spongilla, &c.* Annals and Magazine Nat. Hist., January, 1859; also for July, 1871, *On new Sponges, &c.*

according to the method of Hæckel, to arguing that, since a system of branching canals in the Sponge reminds one very strongly of the intricate network of passage-ways in the basal parts of certain Polyps, therefore the two are homologous, and bear an identical relation to the rest of the organism. Carter (*On new Sponges, &c.*, Ann. Mag. Nat. Hist., July, 1871) has answered this far-fetched homology with considerable detail in a recent paper; and we do not, therefore, feel called upon to add more to it.

The principal aim of this article is to furnish new material in proof of the polycephalism of the Spongiæ, and particularly in regard to their relation with the *Protozoa Flagellata.* We are highly pleased to find that Carter has lately (ut sup., July, 1871) confirmed our earliest observations* as to the organization of the collar-bearing monads of *Leucoselenia*, by an investigation of *Grantia compressa.* He has also accepted our interpretation of the horn-like processes of the sponge-cell of *Spongilla alba;* that they are the outlines of a membraneous collar in profile.

We have now to bring forward a fourth example of a *craspedote*, flagellate monad-cephalid in a Sponge. It seems to be a *Spongilla*, but specifically, at least in its monads, it differs from the English forms. For convenience sake, we will call it *Spongilla arachnoidea*, from its resemblance to an irregular spider-web. It lives in fresh water streams and ponds, usually about the bottom of the stems of water-plants, or wherever there is considerable shade; apparently avoiding the light, as we seldom, if ever, found it in open water. In size it varies from a few inches to half a line in diameter; of no definite shape; and has a uniform fuscous or yellowish-brown color; and is wrapped about by a filmy, transparent, colorless envelope ("*investing membrane*" Carter). The brown color is inherent to the interior mass, in which the groups of monads are imbedded; in fact the latter are themselves as strongly colored by brown granular contents. The "investing membrane" is also slightly tinged with amber color by the large and small spicules which are imbedded in it. Excepting in very small specimens, foreign matter is often so thickly spread over the surface as to obscure the view and seriously interfere with a correct interpretation of the relation of parts. We have been most fortunate in our endeavors with the minuter individuals, which occasionally, we found, would allow a view through and through their entire bulk, and of course left full opportunity for a satisfactory study of the details of special parts, without our resorting to the dissecting needles. Anyone who knows, by experience, the intense contractility of the living sponge, can appreciate the ad-

* Memoirs Boston Soc. Nat. Hist., vol. i, 1867, "*On the Spongiæ Ciliatæ as Infusoria Flagellata.*"

vantage of not being obliged to destroy and sever parts of an organism from their natural relations. Premising thus, that everything has been studied "in place," even to the details of the monads, we shall endeavor to describe this sponge as if it were to be the type for future comparison.

General plan.—The whole individual sponge is endowed with a double envelope (fig. 1 a,a^1, c, d,) the outer and inner parts of which are directly continuous into each other at many points. The outer division (a, a^1) lies at a considerable distance from the monadigerous mass (g), and is, as it were, suspended on the points of the larger, far-projecting spicules (e); just as a tent canvas is supported on the ends of poles. The inner division (c) closely embraces the monadigerous mass like an epidermis, and even plunges between the hollow groups of monads, forming to them a basis of support. The outer and inner divisions are continuous with each other at many points, as stated just now, but only where the larger spicules project. There the envelope (d) runs along the spicules, completely embracing them, as if in a sheath, from their tips to their bases, where they rest on the brown mass of monads. In brief, we might say that the sponge is covered with a miniature colonnade, whose ceiling is the outer division of the envelope, the pillars are the bundles of spicules, and the floor is tapestried by the inner division, which about the pillars hangs from the ceiling in lofty folds. The continuity of the outer division of the envelope is broken by numerous, round or oval openings, of various and frequently changing sizes, sometimes very large, which allow a free ingress of the water to the space just beneath. These are the *afferent ostioles* (os), through and into which a constant current of floating particles may be seen moving with considerable vivacity. Here and there, scattered at wide distances, finger-like, hollow processes from the outer division arise singly, and at various angles. Each is terminated by a large aperture, the *efferent ostiole*, from which a current of water and floating matter emerges with more or less spasmodic irregularity. The smaller individuals, from half a line to half an inch in diameter, possess only one such ostiole; and those an inch in diameter seldom have more than two or three like conduits; but they are very large, sometimes a quarter of an inch in length when fully extended, and of the proportions and taper of the human forefinger.

Plunging the focus of the objective to the floor of the colonnade, the inner division (c) there is found to be pierced by much more numerous openings (i), but far smaller in diameter, and quite methodically arranged, each one corresponding to and overlying a hollow group of monads (h). The outer division is further embellished with irregularly scattered minute

spicules (e^1), which lie imbedded in the cytoblastema, parallel with the surface of the envelope, and occasionally crossing each other at various angles. To complete this general sketch, we will state more definitely the relation of the constituents of the monadigerous mass. There are essentially but two elements here; namely, the inner division (c) of the investing membrane, and the groups of monads (h) which are imbedded in it, below its surface. In a fully expanded individual these groups seldom lie so closely as to touch each other. They vary considerably in size and are usually globular or spheroidal, and form a single *stratum*, with rather narrow interspaces (c^1) between them.

It seems proper here, at least for the sake of precision, that the *cytoblastematous basis*, in which the monad groups are imbedded, should be considered apart from the epithelium-like, inner (c) investing membrane which overlies it, although the two are essentially one; the epithelioid membrane, by prolonging itself between (at c^1) and beneath the groups, forming for them a continuous foundation. In this light, then, we shall speak of the monadigerous mass as consisting of three elements, namely, the inner investing membrane proper, the group of monads, and the *cytoblastematous basis*. This *basis* seems to constitute a large part of the bulk of the body, since it occupies all of the interior space beneath the monad groups. In specimens which grow over flat surfaces in depressed patches, or around stems of plants, it forms a relatively thin layer; but where the body stands out an irregularly rounded mass, sometimes an inch in diameter, the cytoblastematous basis fills up the interior, in enormous proportion to the bulk of the monad layer.

ORGANOGRAPHY.

The Investing Membrane.—The *investing membrane* (fig. 1, a, a^1, c, d,) consists essentially of two histiological elements, namely, a very diffuse *cytoblastema* (a^1) and irregularly disposed cells (b, b^1, b^2) scattered through it. The intercellular *cytoblastema* forms a very thin layer (a^1) between the cells (b); but where the latter are imbedded in it, its outer and inner faces are as wide apart as the considerable depth of the cells demand; and thus it happens that the membrane (both the outer and the inner divisions) presents in profile (a^1, c, d) such an irregular thickness. The *cytoblastema* (a^1) is colorless, hyaline, and apparently homogeneous under a low power; but, when magnified to about four hundred diameters, it displays a very finely granular aspect. It occupies wide intervals between the cells, certainly more than one-half, and fully three-fifths of the whole area of the membrane. Its apparent extent, in a general view, is even more

than that, owing to the extreme transparency of the cells, and their consequent inconspicuousness. That the cytoblastema, notwithstanding its low undeveloped state, is the true contractile element in this membrane, there can scarcely be a doubt, when we consider both its wide spread preponderance, and its relative continuity, as contrasted with the scattered, disconnected condition of the cells (b^2) which are imbedded in it. Sometimes it is barely possible to discover even the trace of a cell on the border of an afferent ostiole (*os*), and in that case we must infer, inevitably, that it is cytoblastema which opens and closes the aperture. We find it, too, embracing the extreme tips of the larger spiculæ, where the cells utterly fail to appear.

The *cell-element* (*b*) of this membrane is also in a lowly condition; only partially developed. There is no *cell-wall*. What may appear to be a wall is really the thin stratum of cytoblastema (a^1) overlying the distal and proximal faces of the cell. This is our conclusion after the most critical scrutiny, with a carefully-corrected objective. Were it not, indeed, for the usually constant presence of a distinct nucleus (*n*) in each cell, we would be strongly inclined to look upon it as merely a dense collection of coarser granules than are generally diffused through the cytoblastemic layer. The irregular and jagged outline, and the caudate projections of the cells (b^2) also tend to tempt one to the latter view. The cell element in this case, then, corresponds only to what is usually considered the cell contents, and a nucleus. The contents are composed of coarse and fine grey granules, which at times are quite conspicuous, but most frequently are so transparent and slightly refractive as to appear, collectively, unless specially focussed upon, as a faint blotch in the investing membrane. This renders it all the more difficult to trace the outline of the cell, and particularly where it throws out irregular, caudate prolongations, to blend with those of other cells. We have been able to detect but one layer of cells in this membrane* when it is well stretched out. The depth of the cells, as may be seen in a sectional profile view (*b*), is about equal to their breadth, and their length is from one-half more than to twice their breadth; but frequently they are as broad as long. They stand in no particular relation to the ostioles; and, as stated above, sometimes scarcely touch their border. The *nucleus* (*n*) may be readily detected by its peculiar, strong refraction, and its considerable superiority in size over the granules. Its bright refractiveness in this connection reminded us of a contractile vesicle, but, although suspecting it of such a function, we could detect no change other than might be produced by the varying length and breadth of the cell, and the

* Carter figures two or three cells overlying each other in *Spongilla alba*. Ann. Mag. Nat. Hist., July, 1857, Pl. 1, fig. 7.

shifting of the relative position of the coarse granules. In the *inner division* (*c*) of the investing membrane the cells are usually smaller than those in the outer division, but differ in no respect, otherwise, neither in form nor arrangement. They lie flat on their sides in the cytoblastemous layer; but, except in profile, they are most difficult to discover on account of the underlying brown mass of monad-groups and granular interstitial substance.

Although we have been unable to discover any distinct cell elements in the cytoblastematous mass immediately around and beneath the monad groups, neither have we found it possible to distinguish it from the cytoblastema lying on the surface; and since the continuity between the two is unbroken, we must, perforce, consider them as one. The underlying portion of the cytoblastematous mass, however, is characterized by irregularly scattered, moderately coarse, brown granules (c^1). These serve very well as a dark frame or setting to the monad-chambers (*h*), and by contrast brings them out more strongly.

The Monad-cephalids.—We now proceed to describe the most essential feature of this animal, the *monads*. They are the characterizing, the dominating element, in reference to which the whole organism is contrived and constructed. They are not cells; they are the *heads* of a polycephalic individual, and consequently correspond functionally to the tentaculated heads of Polypi, and not to their interior epithelial cells. We must first describe what we call the monad-chamber.

The *monad-chambers* (fig. 1, *h*; fig. 2; fig. 4) are deep, spherical hollows which form the receptacles of the groups of monads (*j*). They are mere cavities, and have no lining wall.* They may be easily recognized, in young specimens, as clear, more or less circular, areas scattered in pretty close proximity to each other over the "cytoblastemic mass." Each chamber has a single, small, circular aperture (*i*) which perforates the inner (*c*) investing membrane, and allows egress into the circulatory apartment (*f*). The aperture (*i*) varies in size at times, and may, even, be completely closed. We have never seen it open wider than one-third the diameter of the chamber, and very rarely more than one fifth as wide. That it is a true perforation, and not a clear spot, may be demonstrated by bringing a chamber into profile, so that its aperture (fig. 4, *i*) lies on the extreme border, and then an actual break in the continuity of the investing membrane becomes evident.

*The hollow groups of monads were originally described by Carter (Ann. Mag. Nat. Hist., July, 1857) as lining an hypothecated vesicle, which he named the "ampullaceous sac." He has since (Ann. Mag., Jan., 1859) revoked that view and adopted another. We believe him to be, excepting the inferred "ampullaceous sac," in the main, right in his first interpretation; but as our species are different we cannot speak definitely.

Entering this aperture we do not meet with any obstacle for a little distance around it; there is a clear open space (fig. 4); but pressing onward beyond that, either to the right or the left or directly forward, the cavity appears filled by a collection of vibrating bodies. They seem to be arranged radiatingly from and about the center. Close inspection, however, modifies this view, and it turns out that they are based upon the periphery of the chamber, and converge toward its center, where is a small unoccupied space. We presently recognize these converging bodies to be *craspedote*, *flagellate* monads (*j*), so closely packed together, side by side, as to form a continuous *stratum* (figs. 2 and 4) over the whole concave face of the chamber, excepting immediately about the aperture. Every feature of the monad is strongly marked; even the cylindrical collar is so heavy and conspicuous that its outlines may be seen with as low a power as two hundred diameters. We have studied these bodies with an $\frac{1}{8}$th-inch objective, and found it not at all difficult to focus down upon the details of their organization, without pressing upon or even touching the specimen.

These monads are in every general essential identical with those which we originally found in *Leucoselenia*, and like those, also, recently described by Carter (Ann. Mag. Nat. Hist., July, 1871), in *Grantia compressa*. They are attached to the concave face of the chamber by their posterior end (fig. 4, *j*); and the anterior extremity, with its flagellum (fig. 3, *l*) and collar (*k*), projects freely into the open space, and toward the center of the apartment. When fully expanded, the length of the body and collar together is about one-third, or a little more, of the diameter of the chamber; so that nearly one-third of the latter is unoccupied at the center, except by the tips of the flagella converging from every direction. As the monads lie touching each other on every side (fig. 2), they mutually flatten their bodies, sometimes so much as to give them a strong polygonal outline; or, when the whole mass is expanded, they scarcely impress each other, and therefore retain a rounded contour. By plunging the focus so as to look into the aperture of a chamber, down upon the monads at the bottom (fig. 2) of it, an end view of each cephalid is obtained. From this point the foreshortened cylindrical collar looks like a strong, dark circle (fig 3[a], *k*), which retains its conspicuousness as we plunge down further, even to the base, where it is attached to the body (*j*). The outline of the latter is considerably without the "dark circle," the two being concentric to each other. At the same time we see in the center of the dark circle a black spot (*l*) which may, also, be focussed up and down upon, and hence it is inferred to be a continuous line foreshortened. Other views (fig. 3, *l*) confirm this, and show that it is a single *flagellum*.

The monads are so transparent, and the organization so distinct, that the collar and flagellum may be seen clearly from an opposite point of view, looking directly through the body of the cephalid. This, too, is the best position from which to study the *contractile vesicles.*

A sectional, profile view of a group (fig. 4), to be obtained by plunging the focus half way through a chamber, serves best to disclose the manner in which the posterior ends (*j*) of the monads are affixed to the concave face of their receptacle; and we, also, here obtain a strictly profile aspect of a monad. Figure 3 is such a view, representing a single cephalid, under a much higher power than in figures 2 or 4. An excellent and least obstructed side-view, but not strictly a profile, is to be had by focussing upon the monads immediately about the aperture of the chamber. Here we look directly into the door-way, or through the bordering, transparent epithelioid membrane which it penetrates.

The body, proper (fig. 3, *j*), of a cephalid is a little shorter than it is broad; on the whole spheroidal in shape. Its posterior end is broadly rounded, and so is its anterior extremity, In front arises a cylindrical, membranous "collar" (*k*), which tapers slightly and projects forward to a distance equal to considerable more than twice the length of the body. Its diameter is not more than two-thirds, or even less than that of the body. Although colorless, and homogeneous, it is remarkably conspicuous on account of the thickness of the membrane of which it is composed. Near its open extremity it is more transparent and less obvious than toward its basal attachment.

The *flagellum* (*l*) arises from the center of the anterior end of the body, in the midst of the area which is surrounded by the membranous cylinder (*k*), and without tapering extends a little farther than the open end of the latter. It vibrates usually throughout its length, but is most active near its tip. We have never seen it assume a rigid, arcuate position, as in some other species of monads. It is particularly remarkable for its want of transparency, and looks like a black thread more than any vibrating cilium that we have ever met with. Its action, at times, is rather that of a strong wriggle than a vibration.

The *contractile vesicles* (*v*).—The body of the monad is distinctly marked by a coarse, scattered, brown granulation, with two or three rather large, clear spots, at a considerable distance from each other, but always close to the periphery. These clear areas are the *contractile vesicles* (*v*). They do not occupy any particular place in the body, although they, usually, are not in front. The systole and diastole are extremely slow, but very distinct, if sufficient patience is summoned to watch them fixedly, and without interruption. The last third of the systole is

abrupt, and then only does the vesicle appear to contract suddenly; whereas by watching it through a complete circuit of diastole and systole, one learns that its function is, on the whole, performed very slowly. This very abrupt movement, quite happily, may serve to rebut any such objection as that the otherwise tardy action is merely the result of protoplasmic contraction of the body, as in certain Palmellate Zoöspores. Their immovable position, as regards the body contents, is another item of rebutting evidence.

The *Spiculæ* (fig. 1, *e*, *e*^{1}) are very slender, slightly curved, needle-shaped bodies, gradually tapering to a sharp point at each end. They have a bright amber color, and a rather dark, strongly refractive outline. From tip to tip they are slightly roughened by irregularly scattered, low, but acute prominences or knobs. There are two kinds of spicules, large and small, but they differ in no other respect. The larger (*e*) are from four to six times longer and thicker than the smaller ones. They occur in bundles of two, three or four; and act as props to hold up the outer investing membrane, as described in the early part of this article. They seldom arise perpendicularly from the monadigerous mass, but more or less obliquely; and, in forming bundles, stand across each other like stacked arms. We seldom found spicules penetrating the monadigerous mass far beyond the epithelioid, inner investing membrane. They evidently belong, universally, to the investing membrane, and assist it in forming a framework in which the inner mass is suspended. The *smaller spicules* (*e*^{1}) are strictly confined to the outer division (*a*) of the investing membrane, and lie there on their sides, completely immersed in its thickness. They are scattered irregularly and sparsely about, and frequently cross each other at varying angles. We observe no nearer approach to a methodical arrangement among either the large or the small spicules; yet their very irregularity, being after a kind, and constant in that kind, may be recognized in some sense as methodical.

General considerations.—Seeing the secluded position of the monad-cephalids, deeply ensconced in little chambers below the general surface of the circulatory apartment, it is not directly evident that their *flagella* have any agency in keeping up the inflow and outflow of currents through the afferent and efferent ostioles. Nowhere else are vibrating or non-vibrating cilia or cilia-like bodies to be met with than in the monad chambers. And since the efferent ostioles are irregularly interspersed among the much more numerous afferent ostioles, we cannot conceive how the flagella in any way could influence currents to move in a particular direction, from the smaller apertures toward the larger ones. They no doubt keep up a direct flow

of matter into the sunken chambers, but the current comes from the inner depths of the circulatory apartment, and far away from the ostioles. In this way only a turbulence of floating matter is sustained, but the general, great current is due to a far different cause. We conceive that the contraction and expansion of the body-mass in general, modified by the alternate opening and closing of the afferent and efferent ostioles, is the true motive power in this phenomenon. We have observed, often, that the outer division of the investing membrane is not kept at a uniform distance from the central, monadigerous mass; at one place it will be found to be close to its inner division, so that the circulatory apartment is very shallow there, while at another point the two divisions of the membrane are widely separated, and the circulatory apartment is very deep; and between the shallow and the deep apartments a curtain is drawn, more or less completely, extending from one pillar-like bundle of spicules to another. Each of these temporarily enclosed portions of the general apartment, it is plain now (although our actual observation on this point is very defective), may contract or expand without disturbing the contents of any other. Such an apartment with its afferent ostioles closed, may be contracting and forcing a current out at its efferent ostiole, while a neighboring apartment may have its efferent ostoile closed, and expanding, draw in current through its open afferent ostioles.

We regret that we have not the means, in this locality, for completing these researches. Our specimens were gathered, and studied on the spot where they lived, in the western part of Massachusetts, several hundred miles away from our present residence. Unfortunately we put off the attempt to feed the sponge with colored matter until we had completed other methods of investigation, and then we were prevented, by circumstances, from carrying out our designs.

In regard to the afferent and efferent canals, seen by Carter (Ann. Mag. Nat. Hist., 1857, *ut sup.*) in the monadigerous mass ("*parenchyma*" Carter), we have not met with any trace of them in the species described in this article. It is possible that they may exist in the oldest and largest individuals, but as we worked, only, on very small and transparent specimens, our direct observations, in this respect, strictly apply to the latter. It is more likely that ours is a different genus from the *Spongilla* of Carter, in favor of which we cite the curious fact that each aperture, in the inner division (not mentioned by Carter) of the investing membrane, exactly overlies and is inseparable from the entrance to a monad-chamber ("*ampullaceous sac*;" *partim*, Carter); so that whatever enters these chambers must go out by the same way that it came in; not out into a system of

branching canals, burrowed in the monadigerous mass, but into the great circulatory apartment.

Spongilla arachnoidea Jas–Cl.

DESCRIPTION OF FIGURES OF PLATE II.

The following letters apply to identical parts in all of the figures. *a*, Investing membrane; outer division.—a^1, Sectional profile of the cytoblastema of *a*.—*b*, Cells in the thickness of *a*.—b^1, Cells (like those at *b*) about the spicules (*e*).—b^2, Cells of the investing membrane, with their nucleus; a surface view.—b^3, Temporary junction (by contact only) of the outer (*a*) and inner (*c*) divisions of the investing membrane.—*c*. Investing membrane; epithelioid inner division, in sectional profile.—c^1, Interspaces between monad-chambers.—*d*, Junction of the divisions of the investing membrane along the spicules.—*e*, Larger spicules.—e^1, Smaller spicules.—*f*, Circulatory apartment.—*g*, Monadigerous mass.—*h*, Monad-chambers and monad groups.—*i*, Aperture of *h*.—*j*, Monads, or the body proper in figs. 3 and 3*a*.—*k*, Cylindrical collar of *j*.—*l*, *Flagellum*.—*n*, Nucleus.—*os*, Minor ostioles.—*v*. Contractile vesicles.—

Fig. 1. Magnified 320 diameters. Part of a very young *Spongilla*, of an oblate spheroidal form, and about $\frac{1}{25}$ of an inch in diameter. On the right is presented a face view of the investing membrane and the underlying monadigerous mass. On the left the focus is so adapted as to be fixed on a face-view of the monad-mass, and at the same time on a sectional profile of the investing membrane at a^1, b^3, *c*, and *d*.

Fig. 2. Magnified 780 diameters. Interior of a monad-chamber, seen through the aperture; the monads appear in end view, and crowded together side by side like a pavement work.

Fig. 3. Magnified 1,600 diameters. A single monad, as seen in profile in the monad-chamber. Only two contractile vesicles were present in this specimen. The cylindrical collar (*k*) is extended to its utmost.

Fig. 3*a*. Magnified 1,600 diameters. Foreshortened, front view of a monad; the body (*j*) in the distance; the hollow cylinder (*k*) projecting toward the observer like a dark hoop, and the *flagellum* (*l*) in the center appearing as a black spot.

Fig. 4. Magnified 780 diameters. Sectional view of a monad-chamber, bringing the aperture (*i*) into profile, as well as the monads which lie at the same level; thus showing their convergence about the central open space.

ART. LV.—*Description of a Printing Chronograph;* by G. W. HOUGH, Director of the Dudley Observatory.

ABOUT the year 1848, the idea of recording astronomical observations, by the use of galvanic electricity, was put in successful operation by different individuals. Since that time chronographs of various forms have been constructed for recording in a legible manner, on a moving sheet of paper, the time of any phenomenon observed. The great superiority, in point of accuracy and saving of labor, over the old eye and ear method, formerly used, led to the almost general adoption of the new plan.

During the past ten years the idea of constructing a chronograph, which should print with type the time of the observation, has been entertained by a number of persons. About five years since Prof. Hilgard of the Coast Survey, read a description

of an apparatus designed for this purpose, and about the same time Prof. C. A. Young, of Dartmouth College, published a proposed plan for one.* But, so far as we are informed, the mechanical construction of such an apparatus has not heretofore been attempted by any one.

The construction of a machine which shall carry a type wheel, capable of giving impressions, with uniform velocity for a number of hours together, without sensible variation in its motion, is a problem which is not easy of solution.

Some five or six years ago, in a paper read before the Albany Institute, I gave an account of the method I proposed to adopt, and in the construction of the machine, now to be described, the plan then proposed has been generally followed. My plan, which is radically different from any other proposed, is based on the principle of using separate systems of mechanism for the fast-moving type wheel, and those recording the integer minutes and seconds, regulating each with electro-magnets controlled by the standard clock.

For a clear understanding of the mechanism, elaborate drawings would be necessary. We shall, therefore, merely give a general account of its construction and peculiarities:

1st. A system of clock-work carrying a type wheel, with fifty numbers on its rim, revolving once every second: one, two, or parts of two numbers being always printed, so that hundredths of seconds may be indicated. This train is primarily regulated to move uniformly by the Frauenhofer friction balls, and secondarily by an electro-magnet acting on the fast-moving type wheel, and controlled by the standard clock. This train is entirely independent, and can be stopped at pleasure, without intefering with the other type wheels.

2nd. A system of clock-work, consisting of two or more shafts, carrying the type wheels indicating the minutes and seconds. The motion of this train is also governed by an electro-magnet, controlled by the standard clock, operating an escapement, in a manner analogous to the action of an ordinary clock; every motion of the escapement advancing the type one number.

There are three type wheels, indicating minutes, seconds and hundredths of seconds. The integer seconds are advanced at every oscillation of the standard pendulum; and the minute, at the end of each complete revolution of the seconds wheel.

The type wheels are constructed of brass disks, around the circumference of which is soldered a strip of electrotype copper, holding sixty numbers.

Presuming now we have this system of type wheels in operation, it is necessary to print without disturbing their motion;

* See this Journal, No. 124, July, 1866.

especially is this true for the fast-moving type wheel. After a long series of experiments, during which the fast moving wheel was detached and stopped in various ways, we finally made the impression from the spring of the hammer only, not allowing the blow to fall directly on the type, but arresting it about half an inch before it reached the top of the type. By this device, which is regarded of the greatest importance, the motion of the type is not disturbed an appreciable amount. Any number of impressions following each other in rapid succession, does not disturb the fast-moving wheel the one hundredth of a second. By this plan, none of the type wheels are stopped or locked in the act of printing, and records of observations may follow each other as fast as the hammer can be made to deliver the blow.

If the record is made while the type wheel indicating integer seconds is in the act of escaping, two numbers, or one number and part of another, is printed, so there is never any ambiguity about the record; this condition, of course, only occurs when the fast moving wheel indicates $0{\cdot}95^s$ to $0{\cdot}00^s$. If two numbers are printed when, for example, the hundredths read 98, the smaller of the integer seconds is the correct one. The time required for the action of the escapement is about 0·06 sec.

The blow for printing may be struck directly, by means of a strong electro-magnet; but the cost and trouble of keeping up a large battery for this purpose led us to do all the work mechanically, only using electricity as the governing power. Accordingly, a heavy running gear was built for raising the hammer, capable in its present form of delivering 2000 blows without rewinding; and it can be readily modified to give five times that number, if desirable. This gearing is entirely detached from the hammer when elevated, but is unlocked just before the hammer reaches the type, immediately raising it again. The time consumed for this operation is about three-tenths of a second, allowing, therefore, observations to follow each other at a minimum interval of one half second. When the hammer is elevated it is locked by an electro-magnet, the operation of this magnet allowing it to fall and print. The armature time of the hammer is about 0·07 sec., being but little in excess of our ordinary chronographic recording pen; and since the hammer is acted on by gravity alone, the armature time will be sensibly uniform.

The types are inked by means of small rollers, covered with cloth, resting against their rim, and revolving with the wheel by friction. These rollers require inking every two or three days. If desirable, the inking rollers may be dispensed with, and impression paper used instead. After numerous experiments made with both methods, we have preferred the ink.

The paper fillet, two inches in width, is wound on a small spool, holding about 60 feet, and drawn between two rollers, the same as a Morse Register. Every time the hammer falls, the fillet is advanced about one-quarter of an inch, by the action of an escapement driven by a weight. One spool of paper will hold about 1200 observations, including the spacing for different objects. This same escapement is also operated by an electro-magnet, under the control of the observer, who, by pressing a key, is able to make spaces of any width between the prints.

The train carrying the minutes and integer seconds will run eight hours; the gear for elevating the hammer will deliver 2000 blows; and the train for moving the paper fillet will go 1200 times without winding. The fast moving train runs one hour and thirty-six minutes; but since this train can be stopped at pleasure, without changing the zero of the type, its comparatively brief running is not a serious inconvenience.

To recapitulate, we claim the following:

1st, Separate movements for the integer seconds and the hundredths of seconds; 2nd, The method of regulating the hundredths of seconds wheel by an electro-magnet in connection with the standard clock; 3rd, The method of printing double or single numbers without stopping the type wheels; 4th, The method of striking the blow indirectly, using the spring of the hammer; 5th, The method of elevating and locking the hammer. The minor details for paying off the paper fillet, etc., may be accomplished in various ways.

The battery power required is about the same as for an ordinary chronograph. Three Grove elements, or six Hill's elements, work the two electro-magnets well. A separate battery of about the same size is used for the hammer and fillet magnets.

In point of accuracy, this machine leaves nothing to be desired, and is much beyond what we thought possible. From a vast number of experiments, made by recording automatically the beats of the standard clock, both at the middle and end of the oscillation, the mean error for a single print is found to be about 0·013 sec., equal in this respect to the recording chronograph. The maximum difference in the records of the beats seldom exceeds 0·03 sec.; and we believe this is as much due to the irregularity in the clock connection as in the running of the machine, since the same thing is found in ordinary chronograph records, where the measures are made from second to second.

During the building of the machine, which was accomplished by my assistant, Mr. Foreman, and myself, the past winter, as we could find the time, a great many experiments were tried

in the method of regulation, printing, etc. The fast moving train was used to propel the integer seconds and minute type wheels, dispensing with the auxiliary movement; but the disturbance of its motion was considerable, especially at the end of every minute, when it had double duty to perform.

The saving of time and labor by the use of a printing chronograph is very considerable. At the lowest estimate, it does work equivalent to the labor of one person where three are employed at the same time. In our zone work in former years, when the zone extended two hours in right ascension, it usually required the labor of two persons a whole day to convert the chronographic records into numbers and copy them on the blank forms. With the observations printed, this labor is wholly dispensed with; since the "mean" is at once deduced from the printed records.

The machine is readily adjusted to indicate the same numbers as the clock's face, the type being so set as to print zero-hundredths when the pendulum is at its lowest point, where the magnetic circuit is completed. In the construction of the apparatus, provision was made for attaching engraved rings to the type wheel shaft, showing at a glance the time. But these are not found essential, as they would but little facilitate the setting of the type, which is accomplished as follows: The minute type wheel, which is free to move in either direction, is revolved to correspond to the correct minute; an impression may then be taken, and the machine started, when the clock indicates the same; the seconds being readily counted from the beats of the magnet regulating the fast moving train. The whole time for this adjustment need never exceed two minutes.

In the observation of zone stars, the type may be set to give the integer-seconds of mean right ascension, so that the final reduction will always be a small quantity.

The constant use of this mechanism on every day and observing night, for more than four months, during which time more than ten-thousand records have been made, enables us to speak with confidence of its success, both as regards correctness in printing and in saving of labor.

Other things being equal, it is found, that for three observers twice as many observations can be reduced in the same time, as when a recording chronograph is employed.

ART. LVI.—*Longitude Determination across the Continent;* by GEORGE W. DEAN. (Read before the American Association at Indianapolis.)

WITH the permission of Professor Peirce, Superintendent of the United States Coast Survey, I offer to the Association a brief statement, in regard to the method used, and the results obtained, by the Coast Survey, in determining the longitude of San Francisco and several intermediate points, by telegraphic exchange of clock signals, with Harvard College Observatory, Cambridge, Massachusetts.

In November, 1862, I was requested by the late Prof. Bache to obtain such information as might be practicable in regard to the construction of the telegraph lines across the Continent with the view of determining, by the telegraphic method, the difference of longitude between the Atlantic and Pacific coasts. He at the same time requested me to make a series of experiments with "relay magnets," generally known as "*telegraph repeaters,*" for the purpose of measuring approximately the time required for transmitting a signal through one or more of those instruments. The results of those preliminary experiments were inserted by the Professor in the Appendix to his annual report for 1863, and the final results in his report for the year 1864.

I was greatly indebted to the present distinguished Secretary of the Smithsonian Institution, Professor Henry, for the success attending the experiments with "relay magnets" in 1863–64; and it is gratifying that the results then obtained have, in some degree, aided in modifying and improving the construction of the telegraph instruments now in general use in the United States. In October, 1868, I was directed by the Superintendent, Professor Peirce, to make the requisite arrangements for determining, by telegraphic exchange of clock signals, the difference of longitude between Harvard College Observatory and one of the Coast Survey stations on the Pacific coast, and to prepare a programme for conducting the operations.

The general outline of the plan was to establish an astronomical station at Omaha, Nebraska, which is, by the telegraph route, about 1550 miles from Cambridge. Also a station at Salt Lake City, Utah, which is located about 1050 miles west of Omaha, and 950 miles east of San Francisco. Professor Winlock, Director of Harvard College Observatory, coöperated with the Coast Survey, by placing his astronomical instruments at the service of the Superintendent and directing the telegraphic longitude operations at Cambridge.

The observations for determining the clock and instrumental corrections at Cambridge, were made chiefly by Assistant A. T. Mosman and Sub-Assistant F. Blake, Jr.

At Omaha, the clock and instrumental corrections were determined by Assistant Edward Goodfellow and Mr. E. P. Austin, who used a forty-six-inch transit with an aperture of two and three-fourths inches. All the observations were recorded by an astronomical clock in connection with a chronograph register. At Salt Lake City, the clock and instrumental corrections were determined by myself, assisted by Mr. F. H. Agnew, Sub-Assistant in the Coast Survey.

The instruments used were similar to those provided for the stations at Omaha and San Francisco.

Assistant George Davidson had charge of the longitude operations at San Francisco, and coöperated with Professor Winlock and myself in making the telegraphic longitude determinations across the Continent.

Cold dry weather being most favorable for exchanging telegraph signals between distant stations, arrangements were made for these experiments during the winter of 1868–69.

Whenever the weather permitted, the clock and instrumental corrections at each station were carefully determined, immediately before and after the exchange of clock signals between the several stations.

For this purpose a series of eight or ten standard zenith and two or three circumpolar stars were observed, one-half with the lamp-end of the axis *east*, and an equal number with the lamp *west*. In such a series, the probable error of the result for clock correction in no case exceeded 0·05 seconds. Mayer's formulæ, by application of the method of least squares, have been used in these reductions, which have been made in the most satisfactory manner by Captain Isaac Bradford of Cambridge.

The formulæ and a single example, for the purpose of illustrating the general arrangement of the reductions, are given with the abstract of longitude results.

In closing this paper, I will state, that on the nights of February 28th and March 7th, 1869, the Western Union Telegraph Company, with their usual liberality for the advancement of science, placed two of their telegraph lines, between Cambridge and San Francisco, at the service of the Coast Survey, for the purpose of measuring the "transmission time" of signals sent from Cambridge to San Francisco and returned, and "*vice versa*."

The entire length of the several circuits, which were composed chiefly of No. 9 iron wire, was about 7200 miles, and the number of "*telegraph repeaters*" used was thirteen.

The results were very satisfactory, and accorded closely with the "double transmission time" deduced from the longitude determinations between Cambridge and San Francisco, and also with the results of the experiments made for "transmission time" with a single wire between those points, by Professor Winlock and Assistant Davidson.

Formulæ used in the Reductions.

Star. Lamp Tallies	
α.	
δ.	
M.	Mean of Tallies.
F.	Mean of Thread Intervals.
log F.	
log sec δ.	
log ϱ.	ϱ is the correction for rate, and its log. is 0·000005 for a gain of 1 second daily: 0·00119 for a mean time clock.
log σ.	σ is the sine correction: log σ being additive to log F; when F sec δ is less than 2^m, it may be neglected.
log R.	
R.	$R = F \sec \delta \sigma \varrho$; and is to be added to M to obtain the Time of Transit over the mean of all the threads.
b_0.	b_0 = the level correction in time, corrected for inequality of pivots: it is positive for west end high.
A.	$A = \frac{\sin(\varphi - \delta)}{\cos \delta}$
B.	$B = \frac{\cos(\varphi - \delta)}{\cos \delta}$
C.	$C = \sec \delta$
	$180° - \delta$ being used instead of δ when the star is below the pole.
$\varkappa$.	$\varkappa$ = the diurnal aberration = sec 0·021 cos φ sec δ. It is (−) in upper and (+) in lower culminations.
Bb_0.	
T.	$T = M + R$.
t.	$t = T + Bb_0 + \varkappa$.
ω.	$\omega = \alpha - t = \Delta t + A\alpha \pm Cc$.
Cc.	
ω_0.	$\omega_0 = \omega \pm Cc$, upper sign for lamp west.

The collimation constant (c) is determined from reversals on circumpolar stars, and is to be obtained from the equation

$$t_e - t_w = \omega_w - \omega_e = 2Cc.$$

A is positive except for stars between the zenith and north pole.
B " " " at lower culmination.
C " " " " "

The local time and azimuth are obtained thus: assume an approximate value of the clock correction $= \theta$ for an arbitrary time T_0, and call $w - \theta = w'$; if the collimation is known and the corresponding correction applied, we have only to reduce the value of ω_0 for the several stars to the time T_0 by applying the correction for daily rate. Thus:—

$$(\omega_0) = \omega_0 + \frac{t - T_0}{24 \text{ hr.}} \times \text{daily rate},$$

and we have, putting $\Delta\theta = \Delta t - \theta$,

$$\Sigma\Delta\theta + \Sigma A\alpha = \Sigma\omega'_0$$
$$\Sigma A\Delta\theta + \Sigma A^2\alpha = \Sigma A\omega'_0.$$

Whence we determine α, $\Delta\theta$, and thence Δt for the time T_0.

Reductions of the Observations for Clock and Instrumental Corrections.—February 17, 1869.

$c = +\cdot012$; hourly rate $= +\cdot005$.

Star.	ω Draconis, L. C.		ψ′ Draconis (pr), L. C.		α Orionis.	ζ Geminor.	ε Can. Maj.	δ Can. Maj.	δ Geminor.	α Can. Min.	β Geminor.	φ Geminor.
Lamp	W.	E.	E.	W.	W.	W.	W.	W.	W.	E.	E.	E.
Tallies.	F. C.	E. F.	B. C.	C. B.	C. D. E.	C. D. E.	C. D. E.	C. D. E.	D.	E. D. C.	E. D. C.	E. D. C.
α	hs. min. s. 5 37	s. 41·56	hs. min. s. 5 44	s. 14·35	hs. min. sec. 5 48 5·20	hs. min. sec. 6 30 9·21	hs. min. sec. 6 53 29·63	hs. min. sec. 7 3 4·87	hs. min. sec. 7 12 18·60	hs. min. sec. 7 32 27·46	hs. min. sec. 7 37 18·63	hs. min. sec. 7 45 29·50
δ	+68° 48′	52″·0	+72° 12′	32″·9	+7° 22′ 37″·5	+16° 30′ 22″·4	−28° 48′ 2″·2	−26° 11′ 29″·3	+22° 14′ 8″·4	+5° 33′ 19″·2	+28° 20′ 18″·6	+27° 6′ 2″·0
M	h. m. s. 5 36 24·346	h. m. s. 5 38 34·368	h. m. s. 5 42 49·900	h. m. s. 5 45 19·295	h. m. s. 5 47 52·714	h. m. s. 6 29 56·682	h. m. s. 6 53 17·152	h. m. s. 7 2 52·422	h. m. s. 7 12 6·104	h. m. s. 7 32 14·944	h. m. s. 7 37 6·096	h. m. s. 7 45 16·975
F	+23·499	−23·499	+22·083	−23·519	+0·024	+0·024	+0·024	+0·024	0·000	−0·024	−0·024	−0·024
log F	1·37105	$1\cdot37105_n$	1·344058	$1\cdot37142_n$	8·38021	8·38021	8·38021	8·38021	−∞	8·38021	8·38021	8·38021
sec δ	0·442024	0.442024	0·514927	0·514927	0·00361	0·01829	0·05735	0·04705	0·03351	0·00205	0·05544	0·05051
log o / log σ			·02	·02								
log R	1·813074	$1\cdot813074_n$	1·858987	$1\cdot886349_n$	8·38382	8·39849	8·43756	8·42726	−∞	$8\cdot38226_n$	$8\cdot43565_n$	$8\cdot43072_n$
R	+15·024	−15·024	+112·275	−116·975	+·024	+·025	+·027	+·027		−·024	−·027	−·027
b_0	+0·94	+·066	−·066	+·054	+·054	+·061	+·004	+·010	+·024	+·080	+·076	+·074
A	+2.	580	+2.	976	+·578	+·455	+1·080	+1·037	+0·372	+0·602	+0·276	+0·296
B	−1.	000	−1.	362	+·826	+·938	+0·368	+0·407	+1·014	+0·804	+1·102	+1·084
C	−2.	767	−3.	273	+1·008	+1·043	+1·141	+1·114	+1·080	+1·005	+1·136	+1·123
κ	+·043	+·043	+·051	+·051	−·016	−·016	−·018	−·017	−·017	−·016	−·018	−·017
Bb_0	−·094	−·066	−·090	−·074	+·045	+·057	+·002	+·004	+·024	+·064	+·084	+·080
T	5 37 29·370	5 37 29·344	5 44 2·175	5 44 2·320	5 47 52·738	6 29 56·707	6 53 17·179	7 2 52·449	7 12 6·104	7 32 14·920	7 37 6·069	7 45 16·948
t	5 37 29·319	5 37 29·321	5 44 2·136	5 44 2·297	5 47 52·767	6 29 56·748	6 53 17·163	7 2 52·436	7 12 6·111	7 32 14·968	7 37 6·135	7 45 17·011
ω	+12·241	+12·239	+12 214	+12·053	+12·433	+12·462	+12·467	+12·434	+12·489	+12·492	+12·495	+12·489
Cc					−·012	−0·13	−·014	−·013	−·013	+·012	+·014	+·013
ω_0	+12.	240	+12.	134	+12·421	+12·449	+12·453	+12·421	+12·476	+12·504	+12·509	+12·502
(ω_0)	+12.	246	+12.	139	+12·426	+12·450	+12 452	+12·420	+12·474	+12·500	+12·505	+12·497

February 17, 1869.—*Collimation.*

Star.	Time.	Cc.	C.	c.
	h. m.			
ω Draconis L. C.	5 37	+·001	−2·767	−·0004
ψ' Draconis L. C.	5 44	−·0805	−3·273	+·0246

$T_0 = 6h.\ 46m.$ $\theta = +12{\cdot}550.$

Clock and Azimuth Corrections.—Observer A. T. M.

Star.	Lamp.	(ω_0)	A	ω'_0	A^2	$A\omega'_0$	Aa	Δt
ω Draconis L. C.	W. E.	+12·246	+2·580	−·304	6·656	−0·784	−·312	12·558
ψ Draconis (pr) L. C.	E. W.	+12·139	+2·976	−·411	8·857	−1·223	−·360	12·499
α Orionis,	W.	+12·426	+0·578	−·124	·335	−0·072	−·070	12·496
ζ Geminor,	W.	+12·450	+0·455	−·100	·207	−0·046	−·055	12·505
ϵ Can. Maj.	W.	+12·452	+1·080	−·098	1·167	−0·106	−·131	12·583
δ Cam. Maj.	W.	+12·420	+1·037	−·130	1·076	−0·135	−·125	12·545
δ Geminor,	W.	+12·474	+0·372	−·076	·139	−0·028	−·045	12·518
α Can. Min.	E.	+12·500	+0·602	−·050	·363	−0·030	−·073	12·573
β Geminor,	E.	+12·505	+0·276	−·045	·076	−0·012	−·033	12·538
ϕ Geminor,	E.	+12·497	+0·296	−·053	·088	−0·016	−·036	12·533
			+10·252	−1·391	18·964	−2·452	+	12·535

$$10\,\Delta\theta + 10{\cdot}252\ a = -1{\cdot}391$$
$$+10{\cdot}252\,\Delta\theta + 18{\cdot}964\ a = -2{\cdot}452$$

$$\Delta\theta - {\cdot}1240 = -{\cdot}1391$$
$$\Delta\theta = -{\cdot}015$$

$$\Delta\theta + 1{\cdot}0252\ a = -0{\cdot}1391$$
$$+10{\cdot}251\,\Delta\theta + 10{\cdot}510\ a = -1{\cdot}426$$
$$+ 8{\cdot}454\ a = -1{\cdot}026$$
$$a = -0{\cdot}121$$

$$\Delta t = +12{\cdot}535 \pm {\cdot}007$$

Longitude between Cambridge and Omaha.

[Not corrected for personal equation.]

Date 1869.	$\lambda + \chi_2$ h. m. s.	Probable error.	No. of Series of Signals.	$\lambda - \chi_1$ h. m. s.	Probable error.	No. of Series of Signals.
Feb. 17,	1 39 15·305	±·018	2	1 39 14·990	±·015	3
" 18,	15·293	±·033	2	14·960	±·030	2
" 24,	15 278	±·028	3	14·999	±·032	2
" 25,	15·351	±·015	2	15·009	±·015	2
" 27,	15·351	±·031	1	15·007	±·015	4
" 28,	15·384	±·034	1	14·982	±·033	1
Mean						

Date 1869.	2λ h. m. s.	Probable error.	λ h. m. s.	Probable error.	$\chi_1 + \chi_2$ Double transmission time.
Feb. 17	3 18 30·295	±·023	1 39 15·147	±·012	0s·315
" 18	30·253	± 045	15·127	±·022	0·333
" 24	30·277	±·043	15·138	±·021	0·279
" 25	30·360	±·021	15·180	±·011	0·342
" 27	30·358	±·034	15·179	±·017	0·344
" 28	30·366	±·047	15·183	±·024	0·402
Mean			1 39 15·159	±·008	0·366

NOTE.—Telegraph repeaters were used at Buffalo and Chicago.
Length of telegraph wires in circuit between Cambridge and Buffalo, 504 miles; Buffalo and Chicago, 540 miles; Chicago and Omaha, 498 miles.

Longitude between Cambridge and Salt Lake City.—Not corrected for personal equation.

Date 1869.	$\lambda - \chi_2$		No. of Series of Signals.	$\lambda - \chi_1$		No. of Series of Signals.	2λ		λ		$\chi_1 + \chi_2$
	h. m. s.	Probable error.		h. m. s.	Probable error.		h. m. s.	Probable error.	h. m. s.	Probable error.	Double transmission time.
Feb. 17	2 43 4·497	±·042	1	2 43 3·966	±·030	2	5 26 8·463	±·052	2 43 4·232	±·026	0s·531
" 18	4·609	±·025	2	3·986	±·025	2	8·595	±·035	4·297	±·018	0·623
" 19	4·576	±·022	--	3·985	±·012	3	8·561	±·025	4·280	±·013	----
" 24	4·487	±·035	1	3·982	±·033	2	8·469	±·048	4·235	±·024	0·505
" 25	4·596	±·029	2	3·989	±·028	2	8·585	±·040	4·293	±·020	0·607
" 27	4·563	±·030	2	3·908	±·035	3	8·471	±·046	4·236	±·023	0·655
" 28	4·501	±·041	1	3·827	±·029	2	8·328	±·050	4·164	±·025	0·674
Mar. 3	4·588	±·054	1	4·048	±·040	2	8·636	±·067	4·318	±·034	0·540
Mean									2 43 4·257	±·008	0·591

NOTE.—Telegraph repeaters were used at Buffalo, Chicago, Omaha and Cheyenne.
The length of telegraph wire in the circuit between Cambridge and Salt Lake City was 2583 miles.

Longitude between Cambridge and San Francisco.—Not corrected for personal equation.

Date 1869.	$\lambda + \chi_2$		No. of Series of Signals.	$\lambda - \chi_1$		No. of Series of Signals.	2λ		λ		$\chi_1 + \chi_2$
	h. m. s.	Probable error.		h. m. s.	Probable error.		h. m. s.	Probable error.	h. m. s.	Probable error.	Double transmission time.
Feb. 17	3 25 7·706	±·020	2	3 25 6·917	±·020	2	6 50 14·623	±·028	3 25 7·311	±·014	0s·789
" 18	7·715	±·018	4	6·834	±·031	1	14·549	±·036	7·275	±·018	0·881
" 24	7·629	±·027	2	6·843	±·038	1	14·472	±·047	7·236	±·023	0·786
" 25	7·718	±·024	1	6·865	±·024	1	14·583	±·034	7·292	±·017	0·853
" 27	7·728	±·035	1	6·907	±·023	2	14·635	±·042	7·317	±·021	0·821
" 28	7·615	±·027	2	6·693	±·034	1	14·308	±·044	7·154	±·022	0·922
Mar. 3	7·635	±·042	1	6·859	±·047	1	14·494	±·064	7·247	±·032	0·776
" 5	7·633	±·029	1	6·823	±·029	1	14·456	±·042	7·228	±·021	0·810
" 7	7·637	±·022	2	6·923	±·028	1	14·560	±·036	7·280	±·018	0·714
Mean									3 25 7·260	±·007	0·817

NOTE.—Telegraph repeaters were used at Buffalo, Chicago, Omaha, Cheyenne, Salt Lake City, and Virginia City.
The length of telegraph wire in the circuit between Cambridge and San Francisco was about 3580 miles.

Longitude Determinations across the Continent, January and February, 1869. (Final results.)

Personal Equations.

Dean — Mosman = +0ˢ·11
Goodfellow — Dean = +0·02
} From Gould's Report of May, 1867, p. 75.

Dean — Davidson = +0·180 } From observations made at San Francisco, April, 1869.

Station.	Observer.	Dean's Standard.	
Cambridge,	Mosman,	+0ˢ·110	To be applied to the clock corrections of the respective stations to reduce them to Dean's standard.
Omaha,	Goodfellow,	−0·020	
Salt Lake,	Dean,	0·000	
San Francisco,	Davidson,	+0·180	

To correct the differences of longitude for personal equation, we have:—

Cambridge Time	—	Omaha Time	= +0ˢ·130
" "	—	Salt Lake Time	= +0·110
Omaha "	—	" "	= −0·020
Cambridge "	—	San Francisco Time	= −0·070
Omaha "	—	" "	= −0·200
Salt Lake "	—	" "	= −0·180

STATIONS.	Difference of Long. (λ).	Personal equation.	Corrected difference of Longitude.	Double transmission time.
	h. m. s.	*s.*	*h. m. s.* *s.*	*s.* *s.*
Cambridge to Omaha	1 39 15·159	+·130	1 39 15·289 ± ·008	·336 ± ·015
Cambridge to Salt Lake	2 43 4·257	+·110	2 43 4·367 ± ·008	·591 ± ·019
Omaha to Salt Lake	1 3 49·081	−·020	1 3 49·061 ± ·008	·260 ± ·016
Cambr'ge to S. Francisco	3 25 7·260	−·070	3 25 7·190 ± ·007	·817 ± ·014
Omaha to San Francisco	1 45 52·094	−·200	1 45 51·894 ± ·010	·483 ± ·023
Salt Lake to S. Francisco	0 42 3·024	−·180	0 42 2·844 ± ·008	·242 ± ·016

STATIONS.	Difference of Longitude.	$\chi_1 + \chi_2$
	h. m. s. *s.*	*s.* *s.*
Cambridge to Omaha,	1 39 15·289 ± ·008	·336 ± ·015
Omaha to Salt Lake,	1 3 49·061 ± ·008	·260 ± ·016
Cambridge to Salt Lake (sum),	2 43 4·350 ± ·011	·596 ± ·022
" " (direct),	2 43 4·367 ± ·008	·591 ± ·019
Difference,	·017 ± ·014	·005 ± ·029
Cambridge to Omaha,	1 39 15·289 ± ·008	·336 ± ·015
Omaha to Salt Lake,	1 3 49·061 ± ·008	·260 ± ·016
Salt Lake to San Francisco,	0 42 2·844 ± ·008	·242 ± ·016
Cambridge to San Francisco (sum),	3 25 7·194 ± ·014	·838 ± ·027
" " (direct),	3 25 7·190 ± ·007	·817 ± ·014
Difference,	·004 ± ·016	·021 ± ·030
Omaha to Salt Lake,	1 3 49·061 ± ·008	·260 ± ·016
Salt Lake to San Francisco,	0 42 2·844 ± ·008	·242 ± ·016
Omaha to San Francisco (sum),	1 45 51·905 ± ·011	·502 ± ·023
" " (direct),	1 45 51·894 ± ·010	·483 ± ·023
Difference,	·011 ± ·015	·019 ± ·032
Cambridge to Omaha,	1 39 15·289 ± ·008	·336 ± ·015
Omaha to San Francisco,	1 45 51·894 ± ·010	·483 ± ·023
Cambridge to San Francisco (sum),	3 25 7·183 ± ·013	·819 ± ·027
" " (direct),	3 25 7·190 ± ·007	·817 ± ·014
Difference,	·007 ± ·015	·002 ± ·030

STATIONS.	Difference of Longitude.		$\chi_1 + \lambda_2$
	h. *m.*	*s.* *s.*	*s.* *s.*
Cambridge to Salt Lake,	2 43	4·367 ± ·008	·591 ± ·019
Salt Lake to San Francisco,	0 42	2.844 ± ·008	·242 ± ·016
Cambridge to San Francisco (sum),	3 25	7·211 ± ·011	·833 ± ·025
" " (direct,	3 25	7·190 ± ·007	·817 ± ·014
Difference,		·021 ± ·013	·016 ± ·029
Cambridge to San Francisco,	3 25	7.190 ± ·007	·817 ± ·014
(C. to O.) + (O. to S. L.) + (S. L. to S. F.)		7·194 ± ·014	·838 ± ·027
(C. to O.) + (O. to S. F.)		7·183 ± ·013	·819 ± ·027
(C. to S. L.) + (S. L. to S. F.)		7 211 ± ·011	·833 ± ·025
Cambridge to San Francisco (Mean of all),	3 25	7·194 ± ·006	·827 ± ·012

ART. LVII.— *Notice of the Invertebrata dredged in Lake Superior in* 1871, *by the U. S. Lake Survey, under the direction of Gen. C. B. Comstock, S. I. Smith, naturalist;* by S. I. SMITH and A. E. VERRILL.

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DURING the explorations in Lake Superior, mentioned in the last number of this Journal (page 373) the following species were obtained, together with a number of minute forms, which have not been determined.

A full account of the expedition, with descriptions of the species collected, will be published in the official report of the expedition.

RADIATA.

Hydra carnea Agassiz. A beautiful *Hydra*, agreeing with Ayer's description of this species, was very abundant at the eastern end of St. Ignace, upon rocks along the shore and near the surface, frequently completely covering quite large surfaces where they were protected from the direct sunlight, and was also brought up in many of the dredgings from 8 to 148 fathoms. In 32 fathoms, Neepigon Bay, and in 59 fathoms, off Simmon's Harbor, it was brought up in abundance from a soft clayey bottom. In the deep dredgings, it frequently came up near the bottom of the clay in the dredge, and was evidently not caught while the dredge was near the surface.

MOLLUSCA.

Limnæa. A species allied to *L. disidiosa* Say, was abundant among *Cladophora* in 8 to 13 fathoms on the south side of St. Ignace Island.

Physa heterostropha Say. In the cove at the eastern end of St. Ignace, in 4 to 6 fathoms, and young specimens, in 8 to 13 fathoms, at the locality with the *Limnæa* just mentioned.

Physa vinosa Gould. A very young specimen, apparently of this species, in 6 to 8 fathoms among the Slate Islands.

Planorbis parvus Say. Common in 8 to 13 fathoms on the south side of St. Ignace.

Valvatu sincera (Say sp.). Abundant with the last species, in 8 to 13 fathoms, and also, in 4 to 6 fathoms, in the cove at the eastern end of the same island.

Sphærium sp. nov.? Among the Slate Islands, in 6 to 8 fathoms. A single young specimen of another species of *Sphærium* was found, in 8 to 13 fathoms, on the south side of St. Ignace.

Pisidium Virginicum Bourguignat. On the south side of St. Ignace, 8 to 13 fathoms.

Pisidium abditum Haldeman. With the last species, in 8 to 13 fathoms, and also, in 4 to 6 fathoms, in the cove at the eastern end of the same island.

Pisidium compressum Prime. In the cove at the eastern end of St. Ignace, 4 to 6 fathoms.

Pisidium sp. nov. A small, semi-translucent species, the same as found by Dr. Stimpson in Lake Michigan, was brought up at nearly every dredging. It was common in the cove at the eastern end of St. Ignace, on sandy and muddy bottom, in 4 to 6 fathoms, and abundant among *Cladophora*, in 8 to 13 fathoms, on the south side of that island; among the Slate Islands, in 6 to 8 and 12 to 14 fathoms; at 13 to 15 fathoms on a sandy bottom in Simmon's Harbor; near Copper Harbor, in 17 fathoms, clear sand; in 32 fathoms, very soft clayey mud, in Neepigon Bay; off Copper Harbor, in 62 fathoms, and north of Keweenaw Point, in 82 fathoms, soft reddish clayey mud and sand; and in all the deep dredging down to 159 fathoms.

WORMS.

Lumbricus lacustris Verrill, sp. nov. About 1·5 inches long, ·04 in diameter. Body round, distinctly annulated. Head short, conical, obtusely pointed. Setæ spine-like, strongly curved, acute, arranged two by two, those of each pair close together. Color reddish brown.

South side of St. Ignace, among *Cladophora*, 8 to 13 fathoms.

Sænuris abyssicola Verrill, sp. nov. Worm slender, attenuated posteriorly, about ·30 of an inch long, ·03 in diameter anteriorly. Body composed of about 28 segments, those of the posterior half elongated; those of the anterior half shorter, separated by slight constrictions. Cephalic lobe short, subconical, rounded in front. Mouth large, semi-circular. Intestine slender, moniliform, containing sand. Anus terminal, with three or four slight lobes. Setæ in four, fan-shaped fascicles on each segment, commencing at second segment behind the mouth. The two ventral fascicles are separated by a space equal to about twice the length of the setæ, of which there are five or six in each fascicle; the satæ are simple, acute, slightly curved, equal to

about one-sixth the diameter of the body. The lateral fascicles contain three to five somewhat shorter and straighter simple setæ. One specimen appeared to have four minute ocelli upon the upper side of the head.

Off Copper Harbor, 17 fathoms, sand; off Simmon's Harbor, 60 fathoms; and on the line from the Slate Islands toward Stannard Rock, fourth haul, 159 fathoms.

Sænuris limicola Verrill, sp. nov. Worm more slender than the preceding, attenuated posteriorly, composed of about 44 segments. Length about ·33 of an inch, diameter ·02. Cephalic lobe blunt, conical. Setæ in four fascicles upon each segment, six to eight in each fascicle anteriorly, four or five posteriorly. The setæ in all the fascicles are relatively long, slender, curved and acute. Two tortuous red blood vessels pass along the intestine, forming a loop at each segment. Intestine moniliform.

On the line between the Slate Islands and Stannard Rock, fourth haul, 159 fathoms.

Chirodrillus, gen. nov. Allied to *Sænuris*, but with six fan-shaped fascicles of setæ upon each segment, two of which are ventral, two lateral, and two sub-dorsal; setæ in the ventral and lateral fascicles four to nine, simple, acute, slender, curved like an italic *f;* those of the dorsal fascicles, stouter and less curved, three to six in each fascicle. Intestine wide, somewhat moniliform. Anus terminal, large.

Chirodrillus larviformis Verrill, sp. nov. Body rather short and not very slender, cylindrical, obtuse at both ends, distinctly annulated, composed of about 38 rings. Length about ·30 of an inch; diameter ·05. Cephalic lobe short, conical, obtuse, mouth large, semi-circular beneath. Ventral fascicles of setæ near together, with about five setæ, which are rather short, simple, acute, little curved; lateral fascicles with five or six setæ of similar form and size; sub-dorsal ones similar. When preserved in alcohol, the body is usually curved ventrally or in a simple coil. Color, when living, translucent whitish, intestine slightly greenish. A thickened smooth zone commences behind the 10th setigerous ring, occupying the space of about four segments.

Off Copper Harbor, 17 fathoms, sand; off Simmon's Harbor, 59 fathoms, clayey mud.

Chirodrillus abyssorum Verrill, sp. nov. Sub-cylindrical, thicker anteriorly, distinctly annulated, composed of about 42 segments. Length ·25 of an inch; diameter about ·02. Cephalic lobe short, conical, obtuse, mouth large, semi-circular. Ventral fascicles with eight or nine setæ anteriorly, five or six posteriorly. The setæ are long, slender, acute, strongly curved, those on the inferior side of the fascicles nearly twice as long as those of the upper side; setæ of the lateral fascicles five or six, slender, nearly as long as those of the ventral ones, and similar in form;

dorsal fascicles with four or five shorter, stouter, and straighter, acute setæ.

Six miles S.E. of Passage Island, 47 fathoms; on line from the Slate Islands toward Stannard Rock, fourth haul, 159 fathoms.

Tubifex profundicola Verrill, sp. nov. A rather stout species for the genus, about 1 to 1·5 inches long, ·05 in diameter anteriorly, more slender posteriorly (·02 in diameter). Cephalic lobe short, conical; one specimen apparently had two minute ocelli. Mouth large, semi-circular. Intestine moniliform, with two simple red blood-vessels running along its whole length and uniting at the constrictions. In the first five or six segments there are slender vessels of nearly uniform size, which form lateral loops in each segment. Anus terminal, wide, with about ten small lobes. Setæ in four fascicles upon each segment. Those of the lateral fascicles three anteriorly, often but two, short, slightly curved, mostly with minute forked and hooked tips; those of the ventral series in fascicles of four to six, three or four times longer than the upper ones, considerably bent, the ends minutely hooked and forked.

Neepigon Bay, 32 fathoms.

Nephelis fervida Verrill, sp. nov. Leech two or three inches long, ·20 to ·30 wide, elongated and slender in full extension, very little depressed, most so posteriorly, often round and tapering anteriorly. Mouth large, nearly circular, subterminal, the upper lip, in contraction, short and rounded; corrugated within the œsophagus with three conspicuous folds, eyes eight, blackish, conspicuous, two pairs, a little apart, on the first ring of the head; two pairs wider apart and farther back on the third ring. Color bright brick-red, when living.

In 8 to 13 fathoms, south side of St. Ignace.

A small specimen, probably the young of this species, taken in 13 to 15 fathoms, in Simmon's Harbor, was translucent, tinged with flesh color, with a dark brown intestinal line posteriorly.

Nephelis lateralis Verrill (*Hirudo lateralis* Say). A small specimen, about 1 inch in length, of an obscure liver-brown color, was taken, in 6 to 8 fathoms, among the Slate Islands, which probably belongs to this species.

Ichthyobdella punctata Verrill, sp. nov. Body, in extension, slender, in the preserved specimen, about ·5 of an inch long, ·06 in greatest diameter, rounded, thickest posteriorly, tapering anteriorly to the anterior sucker, which is broad and thin, sub-circular, about three times as wide as the neck where it is attached. Ocelli four, on the upper side of the anterior sucker, the two larger, black ones, in front, and two minute ones wider apart and farther back. Posterior sucker large, rounded or oval. Color translucent greenish, with minute black specks arranged in transverse bands.

Among the Slate Islands, 6 to 8 fathoms.

Procotyla fluviatilis Leidy. Numerous specimens, apparently of this species, were obtained in 8–13 fathoms on the south side of St. Ignace. They were, when living, dirty white, mottled with brown.

In addition to the preceding species of worms, a few were obtained which have not yet been fully determined.

CRUSTACEA.

Mysis relicta Lovén. The occurrence of this and the following species, identical with forms from Lake Michigan, and the lakes of northern Europe, is mentioned in the last number of this Journal. It was brought up with sand and mud from 12 to 14 fathoms at the eastern end of St. Ignace, from 8 to 13 fathoms, with *Cladophora*, on the south side of the same island, and from deep water in a large proportion of the hauls from 73 to 148 fathoms.

Pontoporeia affinis Lindström. This species was found at every haul from the shallowest to the deepest.

Crangonyx gracilis Smith, sp. nov. Eyes slightly elongated, black, composed of few facets. Antennulæ slender, slightly more than half as long as the body; secondary flagellum but little longer than the basal segment of the primary. Antennæ much shorter than the antennulae; the flagellum and peduncle of about equal length, the peduncle being a little longer than the peduncle of the antennulæ. Gnathipoda sub-equal in both sexes, the second pair being only slightly larger than the first; propodus in the first pair quadrate, the palmary margin transverse, nearly straight, and armed with slender spines, of which one or two at the prominent posterior angle are much larger than the others; propodus in the second pair like those of the first, but a little more elongated and the palmary margin slightly oblique. Third, fourth and fifth pairs of pereiopoda equal in length and the margins of their basa spinulose. Ultimate pleopoda reaching to the tips of the penultimate; the outer ramus nearly twice as long as the peduncle, and armed with slender spines; the inner ramus very minute, shorter than the width of the outer. Telson scarcely as long as the bases of the ultimate pleopoda, slightly broader than long, and the posterior margin with a triangular emargination, either side of which the extremity is truncate and armed with several spines.

The incubatory lamellæ of the female are very large, projecting much beyond the coxæ of the anterior legs, as in *C. recurvatus* Grube, which our species much resemble in the form of the antennulæ, antennæ, gnathepoda, etc., while it differs much in the ultimate pleopoda and in the form of the telson. Length, 5 to 7^{mm}.

Among *Cladophora*, in 8 to 13 fathoms, on the south side of St. Ignace.

Gammarus lacustris Smith, sp. nov. Eyes slightly elongated, black. Antennulæ not quite half as long as the body, and furnished with a few short hairs; first and second segments of the peduncle equal in length, third much shorter; flagellum twice as long as the peduncle. Antennæ a little shorter than the antennulæ; ultimate and penultimate segments of the peduncle equal in length, the basal segments short; flagellum considerably shorter than the peduncle. Gnathipoda about equal in size; propodus in the first pair elongated and much narrowed toward the articulation of the propodus; palmary margin slightly concave, continuous with the posterior margin, and furnished, like it, with several stout spines and numerous long hairs, dactylus slightly curved and fully half as long as the propodus; propodus in the second pair a little broader, the lateral margins nearly parallel, the palmary margin somewhat oblique, slightly concave, and furnished with a thin raised margin, and several stout spines, the posterior margin without spines, but furnished with numerous fascicles of hairs. Pleon rounded above, the fourth and fifth segments each with three fascicles of two or three small spines. Third, fourth, and fifth pairs of pereiopoda sub-equal, their basa narrow and the margins furnished with few minute spines. Rami of the posterior pair of pleopoda very slender, the edges furnished with long hairs and a few spines, inner only a little shorter than the outer. Length, 15 to 20^{mm}.

Color in life uniform, obscure, dark brownish-green, without spots or markings of any kind.

Common in company with the last species in 8 to 13 fathoms; also at Simmon's harbor, in 13 to 15 fathoms, and among the Slate Islands, in 4 to 6 and 12 to 14 fathoms.

Asellus tenax Smith, sp. nov. Head broad, with a large rounded sinus in the margin on each side opposite the eye, back of which the margin projects in a rounded lobe, so that the head is not narrower posteriorly than the anterior margin of the first segment of the pereion. Eyes small, prominent, and separated from the margin of the head by more than their diameters. Antennulæ much shorter than the peduncles of the antennæ. Antennæ half as long as the body; the flagellum longer than the peduncle. Propodus in the first pair of gnathipoda narrow and elongated, but considerably stouter in the male than in the female; dactylus more than half as long as the propodus and its palmary edge armed with acute spines, of which the distal ones are larger. The succeeding pairs of legs all similar, the carpal and propodal segments sub-equal in length and armed with short spines along the posterior

edges; the dactyli short, armed with a few spines on the posterior margin, and bi-unguiculate at tip. Pleon narrowed posteriorly, and the extremity obtusely rounded. Posterior pleopoda slender, the outer ramus only half as long as the inner. Length, 8 to 13mm.

Color above dark fuscous, spotted and mottled with yellowish.

Common with the last two species, among the *Cladophora*, in 8 to 13 fathoms; also in 4 to 6 fathoms at the eastern end of St. Ignace, and in 6 to 8 fathoms among the Slate Islands.

Numerous species of Entomostraca were collected at many places, but they have not yet been examined sufficiently for an enumeration of the species.

In addition to the species of the groups already mentioned, insect larvæ and pupæ were obtained at nearly every haul. Several species of *Chironomus*, or of closely allied genera, were common, a slender translucent species being found down to 147 fathoms; an Ephemerid lava occurred at 32 fathoms in Neepigon Bay, and two species of *Phryganeidæ* larvæ were common among *Cladophora* in 8 to 13 fathoms on the south side of St. Ignace.

ART. LVIII.—*On Kilauea and Mauna Loa;* by Rev. TITUS COAN.
(From a letter to J. D. Dana, dated Hilo, Aug. 30.)

DURING the present month I have, in connection with my pastoral labors, visited Kilauea and the whole coast of Puna. I had not seen the volcano since July, 1869, more than a year after the great earthquake and eruption of April, 1868. At this visit, in 1869, I found the crater very quiet. The central and *convex* part had subsided some four hundred feet, forming a vast *concave*, and leaving a high, serrous, black ledge around the circumference of the crater. The south lake was not included in the central basin or depression; but it formed a much deeper pit within the southern rim of the black ledge. The whole crater of Kilauea was then quiescent, with light puffs of long, white steam rising here and there. There were no demonstrations, and so nearly cooled was the bottom of that great south lake—Halemaumau—that I went down into it some *twelve hundred feet below the upper rim of Kilauea*, and measured across the floor. I found the diameter five-sixths of a mile, the pit being more than a mile wide from the upper north to the upper south rim. There were several places, however, at that time, where the incandescent rocks were seen boiling fiercely, through fissures, in caverns fifty to one hundred feet below. Such was the state of the crater on my visit in July, 1869.

On my recent visit, two years later, I found great changes. The south lake had been filled with molten lavas, and successive

overflowings had covered deeply all the southern end of the crater, and sent off their fiery streams two miles to the north, covering the great central depression to the depth of fifty feet.

Mr. L. Kaina, an observant and intelligent Hawaiian, who keeps a respectable hotel at the volcano, told me that, from April to October, 1870 (while I was in the United States), repeated and grand overflowings occurred from Halemaumau, the fiery waves surging and dashing down the northern declivities, filling the whole crater with intense heat and stifling gases; it threw up a sheet of lurid light toward the zenith, producing the appearance of a firmament in conflagration, and reminding one of the bold figures of Peter in speaking of "the heavens being on fire . . . and the elements melting with fervent heat."

My informant told me that he, several times, witnessed what I have more than once seen, viz: the fiery lake rising slowly to the rim, boiling and spilling over, and, by hardening in successive strata, raising a circular dam or barrier around the whole circumference of the lake, some fifteen to twenty-five feet high. Within this circular dike or raised rim the molten sea boils and rages until the heat, the action, and the lateral pressure burst the conical shell, when the seething flood rushes out with awful power, and covers a vast area with its burning waves. Such a scene I witnessed shortly after I lost your pyrometer, which had been thrust into a fiery lake such as is here described. I judge that the overflowings from this south lake, during my absence, cover four square miles to a depth of fifty to three hundred feet—the deepest portion being an elevated region lying all around Halemaumau—and extending east, south, and west, to the outer walls of Kilauea, and flowing down a steep slope to the north, and sweeping over the great central concave.

I was in the crater on the 22d instant, and was at once surprised with the great changes manifest. I had no sooner descended from the northern terrace, or black rim, than I found myself on new ground. All old tracks and landmarks were obliterated. All was *recast*. About half a mile from the south lake I began to rise on an angle of some 25° until I was on a level with the rim of the cauldron. About three hundred yards from the pit the heat was so great, and the gases so pungent, that I could not proceed in a direct line to the margin. Being driven back I made a detour, and again attempted a direct approach. Failing in this, I retreated to a safer atmosphere, and then flanked the fiery pit at some distance, traveling southwestward for half a mile. Here I found the smoke and gases lighter, and again I advanced on the crater at right angles. With difficulty I succeeded in reaching the rim, and a puff of wind favoring by sweeping off the dense smoke of the pit in another direction, I caught a twenty-second glimpse of the awful abyss below. I judged the pit of Halemaumau to be seven hundred feet deep, one mile and a half long from east to west, and one mile from north to south. But it was full of dense columns of smoke and sulphurous gases which were rapidly rising

from burning depths below, and being carried by the winds over all the regions I wished to traverse. Almost immediately after the opening, which revealed to me the profound depths of the fiery abyss, the whole pit was packed and darkened with still denser clouds of smoke, which the fitful wind began to drive upon me. I made a hasty retreat to the northwest, where I reached the original black-terrace, and, mounting an inactive cone some twenty-five feet high, rested in a current of cooler and purer air, and surveyed the marvelous scene I had just left. There is a great body of molten matter in this pit, recognized by its sullen mutterings, and splashings and surgings, and by the bloody glow thrown upon the uprising columns of smoke at night. But the heat and smoke are so great, that it is unsafe to approach the rim of the crater, and usually impossible to see the bottom even could you reach the margin of the pit.

The immense quantities of lava which came from this pit were not all thrown over the upper rim. After this rim had been raised, with all the surrounding area, to a great height, the molten floods formed subterranean passages to the lower parts of Kilauea, where they burst out, and spread seas of liquid fire over the surface.

An active Crater on Mauna Loa.—While at Kilauea, the atmosphere being clear at night, I had a view of an active volcano on Mauna Loa some 13,000 feet high, and about four miles south by west of Mokuaweoweo, the summit crater. This volcano has been burning for weeks, though not often seen at Hilo on account of clouds. Its bearing is about southwest from Hilo, and, as I judge, about north from Kahukuin Kau. It is said that several Hawaiians of Kau have visited it. They say that it is a small lateral crater of Mokuaweoweo, about four miles distant. It has long been extinct, but it now boils in fiery swirls, much like Kilauea. Its action is unequal, sometimes throwing up brilliant jets, and clouds of smoke, and anon seething quietly in its deep caverns. It has not overflowed its rim, or rent its mural walls, or found any subterranean vent by which the molten lake passes off. From week to week and month to month it burns in solitude, and shines like a beacon light upon the dark mountain. Most of the time it is hidden from us by clouds; but when the old mountain—the mother of volcanoes—removes her misty veil, we can then see its distant light.

Some thirty-five or forty miles from Kilauea, and about 9,000 feet above it in altitude, how is it that we can see no sympathy that proves a subterranean connection?

We have occasional shocks of earthquake. From Kilauea I went down to the coast of Puna, and followed its shores to Hilo. The great subsidence of April, 1868, still remains. In most places the people have removed from the shore and built half a mile to two miles inland. In some places they now catch fish where once they cultivated vegetables and grazed their horses. The debris of that little cataclysm is still strewn along the shores of Puna and Kau.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the sensitiveness to light of the haloid salts of silver, and the connection between optical and chemical absorption of light.*—Schultz-Sellack has published with the above title a very interesting memoir, the principal results of which are, in the author's words, as follows:

(1.) In the case of a mixture of chlorine and hydrogen the curve of the chemical intensity of the spectrum which shows the relative chemical action which the different colors exert when completely absorbed, has a course different from that of the curve which directly expresses the observations of Bunsen and Roscoe, and probably more nearly resembles the curve of heat-intensity.

(2.) The sensitiveness to light, determined by photographic excitement, extends in the case of chloride of silver from the ultra-violet to $\frac{2}{3}$HG; in the case of iodide of silver to $\frac{1}{8}$GF; in the case of bromide of silver to $\frac{1}{5}$GF, with iodo-bromide and iodo-chloride of silver to beyond E.

(3.) The dark coloration of haloid salts of silver exposed to the spectrum takes place in the case of chloride of silver within the extent of the photographic excitability; this is probably also the case with the other haloid salts.

(4.) In the case of the haloid salts of silver the absorption of light is always accompanied by chemical decomposition. These salts exert a perceptible absorption upon the spectrum only within the above mentioned limits of photographic excitability.

(5.) Only a small fraction of the absorbed light is converted into chemism. This fraction is different for different colors, but is never zero.

(6.) A thin film of iodide of silver absorbs the light which is more refrangible than G very strongly, the light between G and $\frac{1}{8}$GF but feebly; this last is, however, photographically active. In photographing upon iodide of silver, the interposition of a thin film of the iodide acts like illumination with light approximately homogeneous from G to $\frac{1}{8}$GF.

(7.) At a higher temperature the coloration of the haloid salts of silver becomes deep brown. The sensitiveness to light then probably extends to the red of the spectrum.

(8.) The application of the term *chemical rays* to the more strongly refrangible rays of the spectrum has no other signification than that known substances sensitive to light specially absorb precisely these rays.—*Pogg. Ann.*, cxliii, p. 161. W. G.

2. *On the proteine series.*—Hlasiwetz and Habermann have taken up the study of this somewhat neglected subject, and have arrived at results of much interest. The authors in the first place pass in review the results obtained many years since by the study of the products of the action of various reagents upon members of

the proteine group. These they compare with the products of the action of the same reagents upon the so-called carbon-hydrates, gum, starch, &c., pointing out the close relationship between the two classes of derivatives. The substances selected for examination were caseine, albumen and fibrine, vegetable albumen and legumine. These substances were treated with bromine and water as long as the bromine appeared to exert an action, and the products of the action of bromine separated by processes for which we must refer to the original paper. The results finally reached showed that under the circumstances the products of the decomposition were—

Bromacetic acid,	Asparagic acid,	Leucine,	Tribromamido-
Bromoform,	Malamic acid (?)	Leucimid,	benzoic acid.
Carbonic acid,	Oxalic acid,	Capronic acid,	Bromanil,

together with some ammonia and humus-like substances. Tyrosin was not found in any case; but the authors suggest that it was converted by the action of bromine into bromanil. The products were qualitatively the same for all the substances examined, but quantitatively different, so that the authors infer that the members of the group do not contain a common base like the proteine of Mulder. The action of bromine upon the proteine bodies, like that of other agents of decomposition, yields two classes of products—those which belong to the fatty and those which belong to the aromatic group. Neither of these groups contains more than six atoms of carbon in the principal chain, so that they may be considered as derived respectively from the hydrocarbons, $\mathrm{C_6H_{14}}$ and $\mathrm{C_6H_6}$. Only the highest terms are characteristic of the proteine bodies, namely—leucine, asparagic acid, glutamic acid and tyrosin. All the others are merely derivatives of them.—*Ann. der Chemie und Pharmacie*, clix, p. 304. W. G.

3. *On the products of the reduction of silicic ether and some of its derivatives.*—LADENBURG has studied the action of zinc-ethyl and sodium upon silicic ether. The first product of the reduction is the silico-propionic ether of Friedel and Ladenburg, $\mathrm{Si(OC_2H_5)(OC_2H_5)_3}$. When this is repeatedly treated with zinc-ethyl and sodium, a second product is obtained which has the formula

$$\mathrm{Si(C_2H_5)_2(OC_2H_5)_2}.$$

The density of this liquid is 0·8752 at 0° C., and its boiling point 155°·5. The author terms it silicium diethyl-keton-ether. It is unchanged in the air, insoluble in water, soluble in alcohol and ether. By long boiling with caustic alkalies it is decomposed, and silico-propionic acid may be obtained from the solution. By treating with chloracetyl or chlorbenzoyl the ethyl groups are exchanged for chlorine, while acetic or benzoic ether is formed. Thus in the latter case we have—

$$\mathrm{Si(C_2H_5)_2(OC_2H_5)_2 + 2C_7H_5OCl = Si(C_2H_5)_2Cl_2 + 2(C_7H_5O.OC_2H_5)}.$$

With one molecule of chloracetyl or chlorbenzoyl, the reaction is different; thus we have—

$$Si(C_2H_5)_2(OC_2H_5)_2+C_2H_3OCl=Si(C_2H_5)_2Cl(OC_2H_5).$$

Silicium-diethyl-chlorethin, $Si(C_2H_5)_2Cl(OC_2H_5)$, boils at 148° C.; silicium-diethyl chloride, $Si(C_2H_5)_2Cl_2$, at 129° C. Both are liquids which fume in the air, and burn with a green bordered flame, leaving silica. By the action of water upon the diethyl chloride the author obtained a viscid syrup, the analyses of which agreed tolerably with the formula, $Si(C_2H_5)_2O$. The author considers this substance identical with that obtained by Friedel and Crafts by the action of fuming nitric acid upon silicium-ethyl, and for which they proposed the same formula. By the further action of sodium and zinc-ethyl upon silicium-diethyl keton ether the author obtained an ether having the formula, $Si(C_2H_5)_3(OC_2H_5)$, which he terms silico-heptyl ether, boiling at 153° C. Iodhydric acid reacts with this body according to the equation:

$$2Si(C_2H_5)_3(OC_2H_5)+2IH=(Si(C_2H_5)_3)_2O+2C_2H_5I+OH_2.$$

Silicium triethyl oxide, $(Si(C_2H_5)_3)_2O$, was first obtained by Friedel and Crafts. The new reaction furnishes a method of obtaining it more easily. In conclusion, the author points out a certain regularity in the physical properties of the best known members of the silicium-ethyl series. We have—

		Boiling point.	Density at 0°.
Silicic ether,	$Si(C_2H_5)_4O_4$	166°·5	0·9676
Silico-prop. ether,	$Si(C_2H_5)_4O_3$	158°·5	0.9207
Silico-diethyl-keton,	$Si(C_2H_5)_4O_2$	155°·5	0·8752
Silico-hepty! ether,	$Si(C_2H_5)_4O$	153·	0·8414
Silicium-ethyl,	$Si(C_2H_5)_4$	152°·5	0·7657

—*Berichte der Deutschen Chem. Gesellschaft, Jahrgang* iv, 726. W. G.

II. Geology and Natural History.

1. *Triassic Sandstone of the Palisade Range.*—The Triassic (or Triassico-Jurassic) rock, in New Jersey, as well as Connecticut, is generally a distinct granitic sandstone, that is, it is largely made up of pulverized granite or gneiss. This rock in the Palisade range, New Jersey, is very fine-grained, and has been called *felsite* by Prof. Wurtz, and also by Dr. P. Schweitzer, in a paper on its composition in the American Chemist, July, 1871, p. 23. Five specimens were analyzed, and all were *granular*, and feldspar could be readily distinguished under the microscope. Mr. Schweitzer found the first four to contain ingredients corresponding to 30 to 60 per cent. of albite, showing that the granitic rock out of which these New Jersey fine-grained sandstones were made was albitic. The rock from the Newark quarries consists, according to him, of albite 50·46 per cent, quartz 45·49, soluble silica

0·30, water 1·14, bases dissolved out by hydrochloric acid 2·19= 100·30. It would be of geological interest to find the locality of the albitic granitoid rocks that afforded the material for these sandstones (as we should call them, the term *felsite* being used not for a rock of a peculiar constitution, but rather for one of flint-like or impalpable texture). The Triassic sandstones of the vicinity of New Haven, Ct., contain orthoclase instead of albite, and the material is evidently from the rather coarse granites that lie just west of the sandstone. D.

2. *Martius, Flora Brasiliensis.*—Fascicles 51, 52, 53 and 54 have all appeared during this current year; the first bears the date of February, the last of July, 1871. The enlightened Emperor of Brazil, while in Germany this year (where he visited the grave of von Martius, and plucked and preserved some flowers as a *souvenir*) had the satisfaction of observing the rapid progress of this great national work, under Dr. Eichler's efficient direction. Dr. Eichler has this year been called to fill the chair of Botany at Gratz, with which is combined the directorship of the Botanic Garden. But he is able to carry on the Flora of Brazil, which will still be printed at Munich. With the fascicles now published, title-pages are issued for the volumes or parts of volumes (when the size of the volume calls for a division) already completed. It appears that eight volumes, or rather six half-volumes and two full volumes, are now ready for the binder.

Fascicle 51, by Doell of Baden, commences the *Gramineæ*, but includes only the small tribes of *Oryzeæ* and *Phalarideæ*, the later in the Kunthian rather than the Brownian sense. The *squamulæ* are maintained to be some of them perigonial, but others of stipular nature, i. e., stipules of paleæ. *Oryza* is made to include *Leersia* as a section. *Oryza sativa* and *O. monandra*, *Coix lachryma*, and *Zea Mays* are figured, as also a few rarer grasses.

Fasc. 52, besides Meissner's supplement to *Convolvulaceæ* (Geography and uses) contains the *Cuscutaceæ* by Dr. Progel and *Hydroleaceæ* and *Pedalineæ* by A. W. Bennett of London. Dr. Progel claims for these plants the rank of an order, on the ground of the structure of the embryo, the amount of albumen, and the simply imbricative æstivation of the corolla. Yet in the diagnostic character we read; "*corolla .... lobis .... per æstivationem contortis.*" With good judgment, he refers all to the one genus *Cuscuta*, and gives a conspectus of all the tropical and subtropical American species, after Engelmann. Eighteen Brazilian species are described, one figured at large, and the flowers, seeds, &c., of all of them after the manner of Engelmann's original monograph, which led the way to the present understanding of the genus, and whose more recent and elaborate "systematic arrangement" is almost implicitly followed.

The *Hydroleaceæ* are equally maintained by Mr. Bennett as an independent order, a matter which may almost equally well be decided either way; but we have before alluded to a connecting link between this group and the *Hydrophyleaceæ* which our author

overlooks. Five species of *Hydrolea* and one of *Wigandia* are all that are claimed for the Brazilian flora. The *Pedalineæ* count two genera of a single species each and one of three species.

The 53d fasciculus contains the *Irideæ*, elaborated by Dr. Klatt of Hamburg, and illustrated by 8 plates. It is stated that the known species of the order amount to 470, of which 251 are African, 109 American, 50 Asiatic, 42 European, and 13 Australian. The Brazilian flora counts 57; but there is no *Iris* among them, nothing nearer than a solitary *Cipura*. Nuttall's genus *Nemastylis* has a south Brazilian representative. *Tigridia Pavonia* would appear to inhabit Brazil as well as Mexico, and S. America is the home of *Sisyrinchium*, of which 21 species are Brazilian.

Fasc. 54 comprises the *Escallonieæ* and *Cunoniaceæ*, by Dr. Engler of Breslau, and the *Connaraceæ* and *Ampelideæ* by Mr. Baker of Kew. *Escallonia* counts up to 43 species in Brazil, and *Weinmannia* almost as many. The 35 Brazilian species of *Connaraceæ* are divided among four genera; and there are good figures, with full details, filling seven plates. Of *Ampelideæ* there are 35 species of *Vitis* (taken in the largest sense); and five plates are given. The remaining orders of *Polypetalæ* will soon be forthcoming, and then the *Euphorbiaceæ*. A. G.

3. *Baillon's Histoire des Plants*, in a series of monographs, is making progress, notwithstanding all untoward events. We have before us the monograph on *Menispermaceæ* and *Berberidaceæ*, illustrated by 73 admirable wood-engravings; and that on *Nymphæaceæ*, with 34 figures, both issued in 1871. The illustrations of the *Cocculus* series are taken from *Anamirta Cocculus* (the officinal Cocculus Indicus), our own *Cocculus Carolinus*, and the *Menispermum Dahuricum*, which is very like our own species. The *Cissampelos* series has capital illustrations of the officinal *Cissampelos Pareira*. The *Chasmanthera* series, to which our *Calycocarpum* belongs, is illustrated by *Chasmanthera Columba*, which furnishes the Columbo of Madagascar.

Baillon follows Bentham and Hooker in referring the *Lardizabaleæ*, as a tribe, to the *Berberidaceæ*. But his great point is that he finds in *Erythrospermum* of Lamarck, hitherto placed in the *Bixaceæ*, another syncarpous Berberideous genus, nearly related to the Chilian *Berberidopsis* of Hooker, and with it constituting a new tribe. The name *Bongardia* he replaces by *Chrysogonum* of Rauwolf and Bauhin (which would thus throw out Linnæus's own genus *Chrysogonum*); this going back beyond Tournefort would work endless trouble, if accepted; and by the same rule *Leontice* also should have given place to *Leontopetalum*. *Caulophyllum* is naturally considered as a section of *Leontice*, and all the genera separated from *Epimedium* are suppressed. Baillon has not mentioned the fact announced in this Journal, that *Podophyllum* occasionally exhibits more than one carpel, and he signifies a doubt whether the pulpy investment of the seeds is an arillus. This, however, the fresh fruit makes clear.

The *Nymphœaceæ* are taken in the most enlarged sense, and even are made to include the *Sarraceniaceæ*, which is surely widely beyond the mark. *Nelumbo*, *Cabomba*, *Nuphar*, *Nymphæa*, and *Euryale* are well illustrated; the latter genus is made to include *Victoria;* the quasi-monocotyledonous stems are described, &c. In the first instance the ovules of the *Cabombeæ* are said to be inserted in the internal angle of the ovaries (ventral suture); elsewhere they are more correctly said to be borne on the parieties of the cell; but it is nowhere mentioned that they are more commonly on or near the dorsal suture, and very rarely indeed upon the ventral, as Salisbury and then Brown long ago pointed out.

We should add that this important work, as fast as it appears, is translated into English and published in London, under the name of *A Natural History of Plants.* Reeve and Co. are the publishers. They issue it in monthly numbers, and the first volume is already issued, at 25 shillings; it has 503 wood-engravings.

A. G.

4. *Baptisia perfoliata; the arrangement and morphology of its Leaves.*—In a paper sent by Mr. Ravenel to Prof. Gray, and read by him at the last meeting of the American Association for the Advancement of Science, the character of the torsion of the stem by which the foliage on summer shoots becomes unilateral, is explained. It had been hastily supposed by the present writer that the leaves were five-ranked, and became one-ranked by a continuous torsion of the stem. Mr. Ravenel points out that the phyllotaxis of the plant in question is really of the two-ranked order, which inspection of the growing shoots makes abundantly clear, and that they become one-ranked by the alternate twisting of the successive internodes right and left, i. e., one twists to the right, the next as much to the left, the next in the opposite direction, and so on, thus bringing the leaves into a vertical position all on one side of the horizontal branch. It occurred to Mr. Ravenel that this vertical position of the leaves was correlated with the remarkable alternate torsion of the axis, namely, that the leaves on the reclining branches were adjusting themselves so as to present their two faces as equally as possible to the light, as is done by those of the Compass plant in a different way; and that it was therefore probable that the stomata would be found to be as numerous on the upper face of the leaf as on the lower. A microscopical examination proved the correctness of Mr. Ravenel's conjecture; the stomata are about equally numerous on the two faces. Whether the leaves take a vertical position because the stomata occupy both surfaces, or whether the stomata are so distributed because the leaves stand edgewise to the zenith, is a question. The fact is that the two are thus correlated, and such correlation is ordinarily essential to the well-being of the plant. It may be remarked, however, that the stomata do not manifestly appear until the leaf is pretty well developed. Also that this distribution of the stomata is peculiar to the species in question. At least the leaves of *B. australis* and *B. leucantha*, which retain

their horizontal position, are provided with stomata only on their lower face. The question next arises whether *B. perfoliata* really differs in its normal phyllotaxis from its congener. We find that it does not, that in *B. australis*, *leucantha* and *alba*, and in *B. perfoliata* likewise (these being all the species at present cultivated in the Cambridge Botanic Garden), the arrangement of the leaves at the base of the main stem is of the tristichous order, but that after the first or second cycle, especially on the branches, this changes to the distichous order. The difference between *B. perfoliata* and its congeners, therefore, is not in the normal arrangement of the leaves, but in the fusion of the axis and the distribution of the stomata, adopting the foliage to its vertical position.

The form of the leaves in *Baptisia perfoliata* is remarkably peculiar. Most of the species have trifoliate leaves and a pair of stipules; this has to all appearance a simple and entire perfoliate leaf and no stipules. It is, however, a natural supposition that the apparently simple leaf consists either of a pair of stipules, or of such stipules and a leaflet, connate into a rounded disk. This supposition Mr. Ravenel has just now had the good fortune to verify, by finding some abnormal shorts of *B. perfoliata*, one of which is in our possession. Most of its leaves are cordate-clasping rather than perfoliate, and with or without a retuse or emarginate apex; some almost two-parted so as to represent pretty obviously a pair of stipules; and one of like conformation but with an obvious terminal leaflet in the sinus! Mr. Ravenel remarks that this is a manifest step toward his own *B. stipulacea*. But it hardly invalidates that species, although the inflorescence and legume of the two are quite alike. A. G.

5. *Drosera* (*Sundew*) *as a Fly-Catcher.*—A valued correspondent and accurate observer, Mrs. Treat of Vineland, New Jersey, writes:

"For several summers in succession I have taken *Drosera rotundifolia*, *D. longifolia* and *D. filifolia* from their moist beds, and placed them in sand and water in such a way that they made most charming window-plants. What I take for *D. longifolia* has spatulate-oblong reddish leaves, and long, erect, reddish petioles covered with glands, like those of the leaf. This species I find a much more effective fly-trap than *D. rotundifolia*. On some of the plants in my window this summer almost every leaf held a common house-fly prisoner until it died, and it did not take the leaf very long to fold completely round its victim. My husband was terribly shocked, and thought it the most cruel thing he ever saw in nature; but with my prepossessions and habits, both as an entomologist and a house-keeper, I was contentedly interested to see the work go on."

If we rightly remember, in *D. rotundifolia* it is only the gland-tipped bristles that bend inward and hold the insect fast, while they probably suck the juice out of him. This folding of the blade of the leaf itself around the fly is a new fact to us, and is so especially interesting, being a step toward *Dionæa*, that we

would call particular attention to it, in the hope of further observations and independent confirmation. We are told that the blade incurves from apex to base, in the manner of its vernation. What was long ago known of the action of *Drosera rotundifolia* in fly-catching, had almost completely died out of the books and out of the memory of the present generation until very lately, and the most remarkable things relating to it and to *Dionæa* are not yet in print. A. G.

6. *Borodin: Changes in position of grains of Chlorophyll under Sunlight.*—The singular and prompt change in the position of the chlorophyll grains, discovered by Famintzin and verified by Borodin in Mosses, has been found by the latter to occur in the higher Cryptogamia as well, and now also in Phanerogamous plants, both aquatic and terrestrial. The paper in Bull. Acad. St. Petersb. vol. 13, 1869, is reproduced in the Ann. Sci. Nat., ser. 5, t. 12. *Lemna*, *Ceratophyllum* and *Callitriche* are among the aquatic plants in which the phenomenon has been observed, and *Stellaria media* among terrestrial. *Lemna trisulca* is one of the best plants for these observations. Under diffuse day-light the grains of chlorophyll are distributed over the cell-walls parallel to the surface of the leaf or frond. Under the direct light of the sun they are rapidly (within 15 minutes or less) transported to the lateral walls. There they are at first uniformly distributed. But upon longer insolation, say for three-quarters of an hour, they became grouped in clusters. In darkness the chlorophyll is likewise upon the lateral walls. Thus absence of light produces essentially the same effect as direct sunshine, but less strikingly. Whether these changes are passive and caused by movements of the colorless protoplasm, as Sachs supposes, or active, is not made out. But the movements, according to Borodin, are in response only to the more refrangible rays. A. G.

7. *Dehérain: Evaporation of Water and decomposition of Carbonic acid by foliage.*—Some notice of Dehérain's papers, read before the Academy of Science, Paris, in August and October, 1869, have already been referred to in this Journal. A full abstract published last summer in the Am. Sci. Nat. (ser. 5, tome 12) has come to hand. One of the first and least expected results, which came to light early in the investigation, and simplified the experiments considerably, was:—

(1.) That the transpiration of water continued indefinitely, and quite constantly, in a saturated atmosphere.

(2.) This evaporation, copious in light and almost null in darkness, is determined by the light, and not by the heat of the sun.

(3.) It is much greater from young leaves than from older ones.

(4.) And is mainly caused by the luminous rays (yellow and red).

(5.) The difference in this respect is manifest even when the less refrangible and more refrangible rays are brought to an equal luminous intensity.

(6.) The evaporation of water is much more copious from the upper than from the lower face of the leaf. This result, in view

of the structure of the leaf, and the situation of the stomata, is most unexpected.

(7.) Since the decomposition of carbonic acid also takes effect under the yellow and red rays mainly, and in the upper rather than the lower face of the leaf, the relation between these two capital functions of foliage appears to be intimate and is certainly noteworthy. A. G.

8. *Herbarium.*—The Herbarium of a veteran European Botanist, one of much importance, is offered for sale, the proprietor wishing to see to its satisfactory disposition during his life-time, if possible. Particulars may be obtained from the writer of this notice upon enquiry addressed to the editors of this Journal.

A. G.

III. Astronomy.

1. *Note on the Spectrum of the Aurora;* by Geo. F. Barker. —On the evening of November 9th, there appeared one of the most magnificent crimson auroras ever seen at this place. When first observed, at about a quarter before six, P. M., it consisted of a brilliant streamer shooting up from the northwestern horizon; this was continued in a brilliant red, but rather nebulous mass of light, passing upward and to the north. Its highest points were from 30° to 40° in altitude. A white aurora, consisting of bright streamers, appeared simultaneously, and extended round to the northeast.*

The crimson aurora was examined with the spectroscope at six o'clock. The instrument used was a single glass-prism spectroscope, made by Duboscq of Paris. On directing the slit toward the brilliant streamer above mentioned, a bright spectrum was observed, consisting of five well-marked lines. A millimeter scale attached to the instrument was then illuminated with a gas-flame, the auroral lines being readily measured, even when the numbers on the scale were bright enough to be read distinctly. The sodium line was used to adjust the scale, being equally divided by the division 100; the width of the slit was about one millimeter. As thus arranged, the five auroral lines, beginning at the red end, had the following positions: Scale-Nos. 90, 110·5, 130, 138, 149. The brightness of the lines was, following the above order, 3, 1, 5, 2, 4, the second line from the red end of the spectrum being the brightest. The line marked 90 and the one marked 110·5 were sharp and well-defined; the others were all nebulous on the edges. Before the measurements were completely verified by a second comparison, the crimson aurora entirely vanished, having endured less than a half hour. In the white aurora which remained, the spectroscope showed four of the five lines given; the crimson line

* Professor Newton informs me that he observed an equally brilliant red patch of auroral light in the northeast, five or ten minutes earlier. Since the lower end of the red streamers was much lower than that of the white, it would seem as if the red were seen through the white, the red being most remote.

alone was absent. The measurements are exact to half a division of the scale.

To determine the approximate wave-lengths of these lines comparison was made both with certain elemental lines and with the lines of the solar spectrum. On the scale of this instrument, the elemental lines employed read as follows:—

Kα 63, Liα 79, Srβ 80, H(c) 82, Caα 91, Srα 96, Caβ 113, H(f) 146·5, Srδ 163, Csβ 165, Csα 167, Rbα & β 200, Kβ 218.

The Fraunhofer lines measured as follows:—

a 70·5, B 76, C 82, D 100, E 124·5, b 130, F 146·5, G 189.

Direct interpolation was used in comparing the wave-lengths of the auroral lines with those given above, the wave-lengths of the Fraunhofer and elemental lines being taken from Gibbs's tables (this Journal, II, xliii, 1; xlvii, 194). This method was believed capable of giving results as close as the instrumental measurements. In this way the wave-lengths of the five auroral lines were obtained as given in the following table:—

Line.	Scale number.	Wave-length.	Auroral lines.	Other measurements.
B	76	687		
C	82	656		
(1)	90	623	623	627 Zöllner.
D	100	589		
(2)	110·5	562	562	557 Angström.
E	124·5	527		
(3)	130	517	517	520 Winlock.
b	130	517		
(4)	138	502	502	
F	146·5	486		
(5)	149	482	482	485 Alvan Clark, Jr.
G	189	431		

In this table, column 1 gives the auroral and the Fraunhofer lines; column 2, the number of these as measured upon the scale of the spectroscope used; column 3, the wave-lengths of these lines obtained as above stated; column 4, the wave-lengths of the auroral lines, given by themselves; and column 5, the wave-lengths of what I assume to be the same lines, with their wave-lengths as measured by the observers mentioned.

The point of particular interest in this observation is the fact that the line (4) of wave-length 502 is not laid down in any authority accessible to me as having been observed in the auroral spectrum. Indeed, no previous observer, so far as I know, has seen any auroral line between the Fraunhofer lines b and F. Professor C. A. Young (this Journal, III, ii, 332, Nov., 1871) gives two lines —Nos. 56 and 57, or 1866·8 and 1870·3 of Kirchhoff—observed by him in the sun's chromosphere and also by Rayet in the eclipse of 1868, one of which may coincide with this fourth auroral line.

New Haven, Nov. 13, 1871.

[NOTE.—Since the above was in type, the *Astronomische Nachrichten* for October 24th, No. 1864, has been received. It contains a notice of auroral spectra as observed at the Bothkamp Observatory by H. Vogel, which is dated August, 1871. In ordinary auroras 6 lines were seen, in the red streamers 7. The following are the wave-lengths given:—

629·7	Very bright line.
556·9	Brightest line of the spectrum. Markedly weaker when the red line is also present.
538·2	Very faint line. (Doubtful.)
523·3	Pretty bright line.
518·9	Very bright when the red line is present. At other times, as bright as the last.
500·3	Pretty bright line.
from 469·4 to 462·9	Broad band, brighter in the center. Very weak in the red streamers.

The line of wave-length 500·3 is apparently the same observed by me, and given as 502.

Vogel thinks he has obtained evidence in support of the assumption that the auroral spectrum is an air spectrum, modified by conditions of pressure and temperature.

The whole number of lines which have been seen and measured in the spectrum of the aurora by different observers appears to be 11, as follows:—

No.	Description.	Wave-length.	Observer.
1.	Bright red line	640	H. R. Procter.
		630	H. R. Procter.
		629·7	Vogel.
		627·9	Zöllner.
		623	Barker.
2.	Brightest line in the spectrum	562	Barker.
		557	Winlock.
		556·9	Vogel.
		556·7	Angström.
3.	Band	544	Winlock.
4.	Very faint line	538·2	Vogel.
5.	Band (coronal line?)	532	Alvan Clark, Jr.
		531	Winlock.
6.	Pretty bright line	523·3	Vogel.
		520	Winlock.
7.	Bright line (*b*?)	518·9	Vogel.
		517	Barker.
8.	Pretty bright line	502	Barker.
		500·3	Vogel.
9.	Band (F?)	485	Alvan Clark, Jr.
		482	Barker.
10.	Band	462·9 to 469·4	Vogel.
		464	Winlock.
11.	Band (G?)	434	Alvan Clark, Jr.

Many other observations of auroral spectra have been made, but in most cases the lines were not measured even approximately.

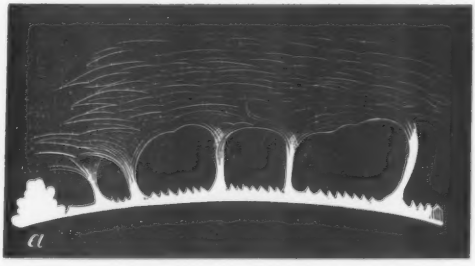
G. F. B.]

2. *An Explosion on the Sun;* by C. A. YOUNG. (Boston Journal of Chemistry).—On the 7th of September, between half past twelve and two P. M., there occurred an outburst of solar energy remarkable for its suddenness and violence. Just at noon the writer had been examining with the telespectroscope* an enormous protuberance of hydrogen cloud on the eastern limb of the sun.

It had remained with very little change since the preceding noon—a long, low, quiet-looking cloud, not very dense or brilliant, nor in any way remarkable except for its size. It was made up mostly of filaments nearly horizontal, and floated above the chromosphere† with its lower surface at a height of some 15,000 miles, but was connected to it, as is usually the case, by three or four vertical columns brighter and more active than the rest. Lockyer compares such masses to a banyan grove. In length it measured 3′ 45″, and in elevation about 2′ to its upper surface—that is, since at the sun's distance 1″ equals 450 miles nearly, it was about 100,000 miles long by 54,000 high.

At $12^h\ 30^m$, when I was called away for a few minutes, there was no indication of what was about to happen, except that one of the connecting stems at the southern extremity of the cloud had grown considerably brighter, and was curiously bent to one side; and near the base of another at the northern end a little brilliant lump had developed itself, shaped much like a summer thunder-head. Figure 1 represents the prominence at this time, *a* being the little "thunder-head."‡

1.



What was my surprise, then, on returning in less than half an hour (at $12^h\ 55^m$), to find that in the meantime the whole thing had been literally blown to shreds by some inconceivable up-rush from

* This is the name given by Schellen to the combination of astronomical telescope and spectroscope.

† The *chromosphere* (called also *sierra* by Proctor and others) is the layer of hydrogen and other gases which surrounds the sun to a depth of about 7,000 miles. Of this the prominences are mere extensions.

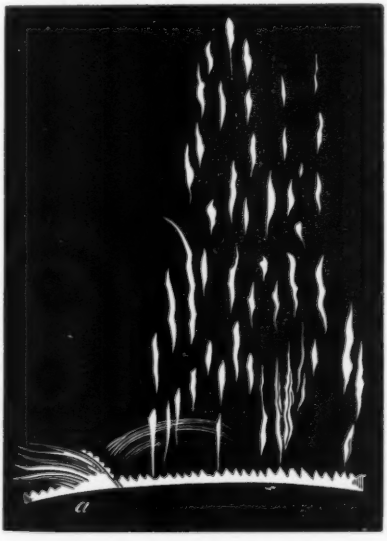
‡ The sketches do not pretend to accuracy of detail, except the 4th; the three *rolls* in that are nearly exact.

beneath. In place of the quiet cloud I had left, the air, if I may use the expression, was filled with flying débris—a mass of detached vertical fusiform filaments, each from 10″ to 30″ long by 2″ or 3″ wide, brighter and closer together where the pillars had formerly stood, and rapidly ascending.

When I first looked some of them had already reached a height of nearly 4′ (100,000 miles), and while I watched them they rose with a motion almost perceptible to the eye, until in ten minutes ($1^h\ 05^m$) the uppermost were more than 200,000 miles above the solar surface. This was ascertained by careful measurement; the mean of three closely accordant determinations gave 7′ 49″ as the extreme altitude attained, and I am particular in the statement because, so far as I know, chromospheric matter (*red-hydrogen* in this case) has never before been observed at an altitude exceeding 5′. The velocity of ascent also, 166 miles per second, is considerably greater than anything hitherto recorded. A general idea of its appearance, when the filaments attained their greatest elevation, may be obtained from figure 2.

As the filaments rose they gradually faded away like a dissolving cloud, and at $1^h\ 15^m$ only a few filmy wisps, with some brighter streamers low down near the chromosphere, remained to mark the place.

2.

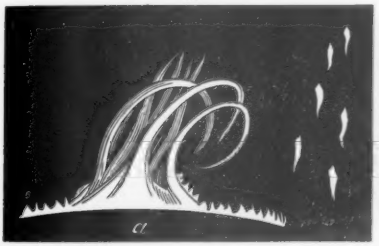


But in the meanwhile the little "thunder-head," before alluded to, had grown and developed wonderfully into a mass of rolling and ever-changing flame, to speak according to appearances. First it was crowded down, as it were, along the solar surface; later it rose almost pyramidally 50,000 miles in height; then its summit was drawn out into long filaments and threads which were most curiously rolled backward and downward, like the volutes of an Ionic capital; and finally it faded away, and by $2^h\ 30^m$ had vanished like the other. Figures 3 and 4 show it in its full development; the former having been sketched at $1^h\ 40^m$, and the latter at $1^h\ 55^m$.

The whole phenomenon suggested most forcibly the idea of an *explosion* under the great prominence, acting mainly upward, but also in all directions outward, and then after an interval followed by a corresponding in-rush: and it seems far from impossible

that the mysterious coronal streamers, if they turn out to be truly solar, as now seems likely, may find their origin and explanation in such events.

3. 4.



The same afternoon a portion of the chromosphere on the opposite (western) limb of the sun was for several hours in a state of unusual brilliance and excitement, and showed in the spectrum more than 120 bright lines whose position was determined and catalogued,—all that I had ever seen before, and some 15 or 20 besides.

Whether the fine aurora borealis which succeeded in the evening was really the earth's response to this magnificent outburst of the sun is perhaps uncertain, but the coincidence is at least suggestive, and may easily become something more if, as I somewhat confidently expect to learn, the Greenwich magnetic record indicates a disturbance precisely simultaneous with the solar explosion.

Dartmouth College, September, 1871.

3. *November Meteors in* 1871.—On the night of November 13th–14th, the writer, with Prof. Lyman and about two others, watched for meteors from $11^h\ 20^m$ onward, with the following results:—

Between	11^h	20^m	and	11^h	30^m	we saw	9	meteors. Sky $\frac{2}{5}$ obscured.
"		30	"		45	"	8	" Fewer clouds,
"		45	"	12	0	"	3	" [but hazy.
"	12	0	"		15	"	8	"
"		15	"		30	"	17	" Nearly clear.
"		30	"		45	"	13	"
"		45	"	1	0	"	15	"
"	1	0	"		15	"	14	" Sky $\frac{1}{5}$ overcast.
"		15	"		30	"	5	"
"		30	"		45	"	6	" Sky $\frac{9}{10}$ overcast.

Shortly after $1^h\ 45^m$ the clouds had entirely closed over, and did not break away afterward. A very few of the 98 meteors seen were of the November meteor system, not more, we judged, than $\frac{1}{10}$ of the number seen. We were therefore at the time of the watch not in the meteor stream. If the earth passed through it this year, it did so earlier than $11^h\ 20^m$, or later than $1^h\ 20^m$, of the night of the 13th–14th. H. A. N.

4. *Asteroid* (117).—This new planet was discovered by Borelly at Marseilles two days before it was seen by Luther. It has received the name *Lomia*.

5. *Tuttle's Comet.*—This comet, first discovered by Méchain in 1790, and again by Tuttle in 1858, and which has a period of 13·6 years, was again seen at Marseilles October 15th, and at Karlsruhe October 15th.

IV. Miscellaneous Scientific Intelligence.

1. *Plattner's Manual of Qualitative and Quantitative Analysis with the Blowpipe*, from the last German edition, revised and enlarged by Professor Theodore Richter, of the Royal Saxon Mining Academy. Translated by Henry B. Cornwall, A.M., E.M., Assistant in the Columbia College School of Mines, New York, assisted by John H. Cadwell, A.M. With 87 wood-cuts and 1 lithographic plate. xv and 549 pp. 8vo. New York, 1872. (D. Van Nostrand, 23 Murray St.).—Plattner's celebrated work has long been recognized as the only complete book on blowpipe analysis. The fourth German edition, edited by Prof. Richter, fully sustains the reputation the earlier editions acquired during the lifetime of the author, and it is a source of great satisfaction to us to know that Prof. Richter has coöperated with the translator in issuing the American edition of the work, which is in fact a fifth edition of the original work, being far more complete than the last German edition.

The American editor, Mr. Cornwall, has done a very great service for all students of chemistry and mineralogy who use the English tongue, in thus adding to our scientific literature a work of such rare merit. He has showed excellent judgment in rendering the work into good English, in avoiding needless repetitions, in adding a large amount of valuable material, and in adopting a mineralogical nomenclature which is familiar to American scientific men.

2. *Geological exploration under* Dr. Hayden.—The geological expedition to the Rocky Mountain region under the charge of Dr. Hayden, after completing the survey of the Yellow Stone Valley, left Fort Ellis on the 5th of September, passing down Gallatin Valley to the Three Forks, and thence by the Jefferson to its very source, exploring many of its branches, and pursuing a direction nearly parallel to that which the party had traversed in the June previous.

The valleys of the Gallatin, Madison and Jefferson forks of the Missouri, with all the little branches, were found occupied by industrious farmers and miners—a contrast quite striking to the doctor, who, twelve years ago, in exploring that same region, met with not a single white inhabitant.

The Rocky Mountain Divide was crossed at the head of Horse

Plain Creek, from which the party passed over into Medicine Lodge Creek, following this down into the Snake River Plain. An interesting fact observed was the occurrence of two species of trout in great quantity in streams such as Medicine Lodge, Comas, and other creeks, all sinking into the plains after a course of from fifty to seventy-five miles. The trout appeared to be of the same two species in all, although the waters had no apparent connection. —X., *in Harper's Weekly.*

3. *Madagascar.*—Few geographical researches of modern times have been more interesting than those carried on in Madagascar by M. A. Grandidier, whether we consider our previous ignorance of the region in question or the number of striking and important scientific novelties brought to light. We have already referred to the return of this gentleman from his third expedition, the first having been commenced in 1865. On this occasion he attempted to reach the heart of the island, but in vain; and in the following year he explored the southern region, but did not reach the mountains. In 1869–70, however, he traversed the entire breadth of the island three times, from west to east, through its whole extent, making various lateral excursions to interesting points, and visiting the peak of the mountain Ankaararatre, the highest summit in Madagascar. According to the report just presented by M. Grandidier to the Academy of Sciences of Paris, Madagascar comprises two distinct regions—the northern, which is mountainous, and that to the south and east, which is flat. He ascertained that there are five chains of mountains, which have generally the same direction —namely, from northeast to southwest. These are separated by sandy and arid plains, intersected by shallow ravines. After crossing the fourth chain, a region is reached of which the general level is from 1000 to 1200 meters in height, extending to the Indian Ocean, a vast sea of mountains, with no level lands except a few small valleys used for the cultivation of rice.

The eastern coast is intersected at almost every step with rivers and torrents; and the northwestern provinces pour into the sea a large number of important rivers. On the southern and western regions, however, the case is quite different, there being distances of fifty leagues without the smallest brook. The reputation possessed by Madagascar for luxuriant vegetation and fertile soil, according to M. Grandidier, is by no means merited, its provinces being neither rich nor productive. The secondary plains are sterile, and the population is confined to the immediate banks of the water-courses. The entire mass of the granitic mountains, situated to the west of the eastern slope, is naked and arid, and there is no vegetation excepting here and there little bunches, growing in the ravines. In the opposite direction, however, there is some degree of fertility; and there is a line of forests extending from north to south, which connect with those of the west, forming around the island a narrow girdle, including a dry and desert region in its center.

M. Grandidier made numerous astronomical, meteorological, and magnetical observations. He also studied closely the ethnology of the inhabitants, having taken a great many measurements upon the living body, and having collected notes of the habits, language, and traditions of the people. His natural history collections embrace over fifty new species of vertebrates, together with numerous insects and plants. Large numbers of alcoholic specimens were also gathered, for the purpose of further investigation into the anatomy and structure of the entire animal.—X., *in Harper's Weekly.*

4. *Dr. A. Habel.*—After a seven years' tour of exploration in South America, Dr. A. Habel, a former resident of Hastings-on-the-Hudson, has returned to New York, where he is assiduously engaged in preparing the results of his labors for the press. Among the regions traversed by this gentleman may be mentioned the greater part of Central America, the Cordilleras of the Andes in Colombia, Ecuador, and Peru, and finally the Chincha Islands and the Galapagos. During this whole period Dr. Habel was diligently occupied in gathering information in regard to the natural and physical history of the countries mentioned, especially in the departments of ethnology, meteorology, and zoology. He has already made some communications on the subject of his travels to the Academy of Sciences at Paris, and other learned bodies, and we look forward to his detailed report with anticipations of much interest. The guano deposits of Chinchas were thoroughly explored by the doctor, who found them to be of a much more complicated structure than has hitherto been supposed.—X., *in Harper's Weekly.*

5. *On Sea Waves;* by W. J. Macquorn Rankine, C.E., LL.D., F.R.S. (Royal Institution of Great Britain, May 26, 1871.)—The speaker in the first place gave a summary, illustrated by diagrams and machines, of the existing knowledge of the mode of motion of water in waves, and of the geometrical and dynamical laws which govern the relations between the depth of disturbance of the water, the velocity of advance of waves, their periodic time, and their length. He referred to the experimental and theoretical researches of previous authors on the subject, such as Webers, Airy, Scott Russell, Caligny, etc.

He then explained the principle, of which Mr. Froude was the first to point out the importance, that the action of water agitated by waves upon a ship tends to make her perform the motions which would have been performed in her absence by the mass of water that she displaces. In still water, the forces of gravity and of buoyancy tend to keep the ship upright, and if she has been heeled over, to restore her to the upright position, and that tendency constituted the *statical stability* or *stiffness* of the ship. Among waves, the same forces, combined with the reactions due to the heaving motions of the water and of the ship, tend to place her in the position called *upright to the wave surface;* that is, with her originally vertical axis normal to the wave surface. If the ship

yielded passively to that tendency, like a broad and shallow raft, she would accompany the waves in their rolling; and thus, a ship having great *stiffness* may be very deficient in *steadiness*. Every ship has, like a pendulum, a natural period of rolling, depending on her stiffness, or tendency to right herself, and her moment of inertia, being a quantity depending on the distribution of her mass. Stiffness tends to shorten, and inertia to lengthen, the period. It was shown in 1862, by Mr. Froude, that the greatest unsteadiness and the greatest danger of being overturned takes place when the periodic times of rolling of the ship and of the waves are equal; for then each successive wave adds to the extent of roll; and if the coincidence of the periods were exact, the ship would inevitably be overturned in the end.

In the course of the present spring it has been pointed out that in well-designed ships a safeguard exists against the occurrence of such disasters. It is well known that no pendulum is absolutely isochronous; but great oscillations occupy a longer time than small oscillations. In like manner, no ship is absolutely isochronous in her natural rolling; but great angles of roll occupy longer periods than small.* Hence, supposing a ship to encounter waves of a period equal or nearly equal to her own natural period for small angles of roll, her angle of rolling is at first progressively increased; but at the same time her natural periodic time of rolling is increased, until it is no longer equal or nearly equal to the periodic time of the waves; and thus she in a manner *eludes* the danger arising from coincidence of periods. In order, however, that this safeguard may act efficiently, it is essential that the natural period of the ship for the smallest angles of roll should not be less than the period of the waves; otherwise the first effect of the progressive increase of angle will be, not to destroy, but to produce coincidence of period; and the result will be great unsteadiness of motion, and possibly great danger.

The speaker described the above principles as being the latest additions to our knowledge of the theory of the relations between ships and sea-waves; and he illustrated them by means of experiments on a machine so constructed as to imitate the dynamical condition of a ship rolling among waves.

6. *On a Meteor seen at Alexandria, Egypt;* by BEVERLY KENNON, Colonel Coast Defense, Egyptian Army. (From a letter dated Alexandria, Egypt, Sept. 9th, 1871, addressed to Rear Admiral Sands, U. S. N., and by him communicated for this Jour-

* An exception to this rule exists in the case of that form of ship known as the "Symondite," in which the sides flare out at and near the water-line, so as to make the stiffness increase faster than the angle of heel. In such ships the period of rolling *shortens* when the angle increases; and thus the well-known unsteadiness of large vessels of that model is accounted for. In a small boat, whose natural periodic time for the smallest angle of roll is shorter than that of any of the waves which she encounters, the Symondite model does not promote unsteadiness; for the shortening of the natural period of rolling removes it farther from coincidence with the period of the waves.

nal.)—The meteor attracted my attention last night (Sept. 8th) and also that of my companions, and many other persons who were in the café on the sea-shore with me when it passed us in its flight. It was very large and bright, and starting from the zenith moved nearly due north, and exploded or disappeared when about 20° above the horizon, and about S.E. from the North Star. It moved leisurely and disappeared apparently behind a cloud; when watching for its reappearance, we were astonished to find that the luminous streak which marked its passage appeared to remain stationary from the point at which the meteor disappeared. As it maintained its brilliancy for some seconds, I noted the time by watch; it was then $11^h\ 14^m\ 30^s$ P. M. M. A. time. For one minute and ten seconds this brilliant streak, stretching toward the northern sky, and subtending an angle of about six degrees, remained perfectly straight and very bright; at the expiration of another minute it had changed its shape into that of a curve, convex toward the earth; at the end of two minutes and thirty seconds it was obtuse-angled, and of moderate brilliancy; at the end of the third minute it was right-angled in shape, and of a distinctness sufficiently great to attract attention, disappearing entirely after three minutes and twenty seconds from the time we first noticed it.

7. *Kansas Academy of Sciences.*—The Kansas Natural History Society, of Leavenworth, Kansas, changed its name to that of the Kansas Academy of Sciences, at its fourth annual meeting on the 25th and 26th of October. The officers of the Academy are Gen. John Fraser, President; B. F. Mudge and Robert J. Brown, Vice Presidents; John D. Parker, Secretary; F. H. Snow, Treasurer; B. F. Mudge and F. H. Snow, Curators. Several papers were read at the meeting on the Natural History and Geology of Kansas and on other topics.

8. *The Fossil Plants of the Devonian and Upper Silurian of Canada*, by J. W. Dawson, LL.D., F.R.S., F.G.S. Geological Survey of Canada, Alfred R. C. Selwyn, F.G.S., Director. 100 pp., with 20 plates. Montreal, 1871. (Dawson Bros).—Dr. Dawson has here brought together the results of his long labors on the fossil Paleozoic botany of Canada, and made a volume of great value to Geology and to science in general. Its twenty plates are crowded with excellent figures. The closing chapter, forming a "supplementary section" to the volume, is republished on page 410 of this number. The memoir is worthy to take its place along side of the other Paleontological publications of the Canada Geological Survey.

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